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JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

DEVOTED TO

MECHANICAL AND PHYSICAL SCIENCE,

Civil Engineering, the Arts and Manufactures.

EDITED BY

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JULY, 1864.

CIVIL ENGINEERING.

On the Organization of the Corps Imperial Des Ponts-et-Chaussées, in France. By George R. Burneel, Esq., C.E., F.G.S.

From the Journal of the Society of Arts, No. 591.

As England is approaching the time when the services of the state are destined to be more or less centralized, it seems to me necessary for those who are charged with the superintendence of the various offices of the government to acquaint themselves with the principles upon which foreign nations have organized the bodies to whom they have entrusted the peculiar duties connected with the various branches of the administration. There is, too, a tendency on the part of some of our statesmen of the present day to praise everything that emanates from the French government, particularly with respect to the administration of public works; and as the Corps Imperial des Ponts-et-Chaussées is the most important body that the Minister of Public Works employs upon them, it seemed to me that it would afford the subject of an evening's discussion that would be fraught with interest to you, were I to relate succinctly that which I know with respect to the organization of that body, and of the education that the members of it are obliged to go through.

The public works of France constitute the object of a separate department of the administration of the country, under the control of a minister, who is responsible to the Emperor for the manner in which he discharges his duties. These consist of the maintenance and the perfection of the means of internal communication, both by land and water; the providing for the safety and convenience of the traffic with foreign countries, by the docks, river navigation, the lighthouses, and beacons of the coast; the works that are undertaken for the improvement of agriculture, such as draining and irrigation works, &c.; the mining operations (by the way, these operations are conducted under the superintendence of a special body of engineers, called the Ingénieurs des Mines, with respect to the organization of whom I read a paper at the meeting of the Royal Cornwall Polytechnic Society, in the month of September last); the conditions that are to be observed with regard to the exercise of unhealthy and dangerous trades; the working of steam engines; the erection and maintenance of the buildings of the state, beyond those that are comprised within the functions of the other ministers; but in many cases the Ingénieurs des Ponts-et-Chaussées enter into the service of the state under the different branches of the administration, as *en service détaché.* The Minister of Public Works has also to prepare and propose the laws for the regulation and control of private commerce; for the encouragement of arts, so far as they are effected by the processes of manufacture. He has the control of the industrial schools, the fund for the superannuation of government employees, the saving banks, the joint stock companies, the private banks, and the superintendence of the service of weights and measures. He is also charged with ascertaining the changes in foreign legislation on the subjects that are likely to interest French commerce, and it is his duty either to publish them, or send them round to those that may be effected by them; according to the regulations that prevailed some time since, he had the superintendence of the general statistics of France, and of the government stations for breeding horses. In the discharge of these duties he is assisted by the Ingénieurs des Ponts-et-Chaussées, for all that relates to the services that come within their functions: thus there is a Direction of Roads and Bridges, that is charged with the surface of the highways, and the departmental roads, and with the police of the traffic upon them. There is a Direction of Navigation, that is charged with the service of the ports and the canals, with the rivers, both navigable and floatable, understanding by the latter word such as are capable of floating down timber, but are not suited for navigation, either on account of their velocity, or their shallowness, or irregular volume; with the police of these rivers, and with the arrangements for their improvements. There is a Direction of Railroads, divided into two sections, of which the first undertakes the regulation of the lines and their concessions; and the second undertakes the works, and the direction of the working of the railroads, and the collection of the statistics of that means of locomotion. The Minister is assisted by the Ingénieurs des Mines in their department, but they are immediately under the orders of the secrétaire-général of that body; in the other branches of his duties the minister is assisted by a numerous staff of officers that relieve him of the care of the details, and allow him to concentrate his attention upon the broad principles involved in the policy he himself is disposed to carry into effect. It is probable that in the rapidly changing system that now prevails in France, some portion of the duties that are above enumerated, as, for instance, the superintendence of the horse-breeding establishments, may be withdrawn from the functions of the Minister of Public Works, Agriculture, and Commerce; but the list given of the minister's duties was the correct one some time since, and it contains, at any rate, a correct statement of the functions that the French authorities considered to fall within the limits of that officer's occupation.

The Administration des Ponts-et-Chaussées, it is thus seen, forms one of the most important branches of the service of the Minister of Public Works, and is organized in the most systematic manner, according to the French point of view, to secure the effective discharge of the services entrusted to it. The date of the creation of this body is rather remote, and it is another illustration of the manner in which the *Ancien Régime* had prepared the way for the Revolution. As far back as the time of Henry IV. there existed, in fact, a government body of engineers, under the direction of the Grand Voyer of the Kingdom. Louis XIV. gradually increased the power of these engineers; and in 1722, the Regent instituted the Corps des Ponts-et-Chaussées, under its present name, and with nearly the same functions that it has at the present day, for the purpose of executing the system of public works that France had then determined upon. MM. Trudaine, father and son, were the heads of this organization, and they were succeeded by Perronet, in the direction of the Ecole des Ponts-et-Chaussées at any rate, about the year 1750; at the same time the local governments of Languedoc and Bretagne established their separate bodies of engineers on the same footing as that of Paris for the rest of the kingdom. In 1791 the Constituent Assembly passed a decree, maintaining the organization of the Ponts-et-Chaussées, but they introduced into it the system of management that now prevails, and they only recognised one school for it, viz: that of Paris. There have been some changes, without importance, since that period, as in the years 1804, 1817, 1836, 1839, 1853, 1855; but substantially the organization of the administration remains the same as it had been settled by the Constituent Assembly. The acts that have been referred to only made some alteration in the ministry that they were under, and a little difference in the authority of the sous-secrétaire d'état that was specially charged with the direction of the united bodies of the Ingénieurs des Mines, et des Ponts-et-Chaussées.

The minister, then, under the present system approves the projects of the new works submitted to him in the regular course of his business, and those of the great repairs of the roads, bridges, canals, and harbors; he sees to the regularity of the contracts that are taken; he divides the credits allowed by the legislature; and he prepares the decrees regulating the position of the engineers from the second class, observing in all cases the regular system of promotion, unless there be some very peculiar circumstances connected with the case. The directeur-général has the superintendence of the correspondence with the préfets of the various departments and the engineers; he presides over the meetings of the conseil-général in the absence of the minister; and he prepares all the details of administration that the minister is required to sign. The conseil-général is composed of the engineers of every degree that may be present at the time in Paris; it is presided over by the minister; in his absence, by the directeur-général, or by an inspecteur noninated for the purpose by the minister. It has to pronounce upon the projects and plans of works, and all the questions relating to construction that may arise; upon the questions of accounts, and upon those that concern the legal rights of the state, arising out of the property that it has in the works executed; and, finally, it decides the questions that are connected with the duties of the engineers, that would have to be brought before the council of state or the minister; for in France no person in the employ of the government can be prosecuted for the acts that he may commit in the course of the discharge of his duty, however gross they may be, without the permission of the conseil d'état before whom the actions that may arise must be tried.

The whole of France is divided into eighteen districts, each of which is under the inspection of an engineer, who receives the title of inspecteur-général de 2nde classe; the inspecteurs-généraux de 1re classe being honorary members, and without any definite functions. The inspecteurs are obliged to visit their districts every year, for the space of three months, and they render an account to the conseil-général of the state in which they have found everything in the course of their visit, either with regard to the composition of the staff, the manner in which the accounts are kept, or the conditions they may observe to prevail in the works, such as the roads, bridges, rivers, canals, harbors, railways, factories, ferries, drainage operations, &c. This report must be prepared on paper that allows the engineers in charge of the various parts of the public works to append their remarks to it; or rather the latter have to make a report that is transmitted to the conseil approved and commented upon by the inspecteur; the reports of the latter class of officers, however, that relate to the staff of engineers, are sent directly to the minister, on account of the confidential character of the communications. The service of the departments is divided between the ingénieur-en-chef de 1re classe and the ingénieurs-en-chef de 2nde classe; the ingénieurs-ordinaire de 1re, 2nde, et de 3me classe; and the conducteurs faisant fonctions d'ingénieur-ordinaire, or of the various brigades of the officers of that rank. The ingéniéur de 1re classe is charged with the preparation of the projects for the improvement of his district; the demand for, and the opening of credits; the execution of works either by competition or by régie, which means that the state employs the workmen and engages the tradesmen to furnish materials on its own account; the number of people employed in the offices; the payment of the accounts; and the delivery of the certificates and orders upon the treasury; the direction of the law proceedings; and the movement of the *employees* of the office. The ingénieur-ordinaire presents to the ingénieur-en-chef the accounts of the measurement of the works executed, and the various conditions of the reception, and the state of those works; he is also charged with the examination of the projects that it may be considered advisable to undertake, which are then submitted to the ingénieur-en-chef, and by him are transmitted to the ingénieur-inspecteur, to be by him co-ordinated with the general projects for the whole kingdom. The ingénieurs-ordinaires are likewise charged with the duty of following the inquiries before the local authorities, into the propriety of establishing certain trades in the localities, that may give rise to question *de commodo et incommodo*, as the French style the questions that are connected with the establishment of unhealthy trades, and the regulation of the lines of buildings on the public roads; the police of the roadways is also within the jurisdiction of the ingénieurs-ordinaires, who are, however, obliged to conform to the orders they may receive from the higher authorities in these matters. It is the duty of the ingénieurs-ordinaires to see that the various contractors execute their engagements with the state in the strictest manner, and they must personally superintend the measurement of the various accounts they certify; they are assisted in the performance of their various duties by the conducteurs, who may, on some occasions, be promoted to the fulfilment of the office of ingénieur-ordinaire; but the rule of the promotion in the Corps des Ponts-et-Chaussées is very much opposed, as we shall see hereafter, to the irregularity of this movement of the last-mentioned body of men; they only are allowed to perform the functions of engineer, whilst they remain conducteurs to the end of their lives.

The Ingénieurs des Ponts-et-Chaussées are admitted into that body in the following manner, that is never departed from, let the candidate for distinction display ever so much knowledge that would become useful in the profession of a civil engineer. The candidate for admission must first of all prove himself capable of going through the examination of the Ecole Polytechnique, and if he support that ordeal, he must study in that school for the space of three years, passing through the examinations that the pupils of the Ecole are subject to every year. He then, after going through another examination, proceeds to the special school of the Ponts-et-Chaussées, where he follows a course of education that is considered to be such as to qualify him to practise as a civil engineer. He has to go through various repetitions and preliminary examinations, that in their variety of subjects, and the profundity of the scientific information that they require, would sorely puzzle the English engineers in the majority of cases; and finally, at the end of three years, the candidate is allowed to pass the final reception, and to be classed as an aspirant-ingénieur, or an ingénieur de 3me classe. The pupils who go through the last examinations with particular distinction are sent abroad to study the peculiarities of foreign practice; and when they return their notes are published, for the most part, in the "Annales des Pont-et-Chaussées." They are then sent with their colleagues to the various places that may be in want of an engineer; but their special talent is hardly considered in the choice of their pursuit, either in their first nomination or in their subsequent promotion, which follows the regular steps, and leaves little room for the display of original talent of any kind. The consequence of this organization of the school is that the young men leave it with a vast amount of undigested knowledge that would enable them to pass the examinations in the various branches of their profession; but they rarely possess any practical knowledge whatever. "They are," as M. Etex said in his lectures "On the Fine Arts applied to Industry," "capable of anything and fit for nothing;" and the fact that their promotion in after life depends upon the mere principle of their seniority, tends very much to perpetuate the sort of feeling which the pupils of the Ecole des Ponts-et-Chaussées have when they leave that institution. This may be said to be a perfect self-satisfaction with themselves, such as always accompanies the profession of theoretical as contrasted with practical knowledge, and a contempt for all who may not be so well acquainted with the class of information that they have been enabled to obtain; and there is this peculiar disadvantage attending the kind of preparation for the future exercise of their profession that the young men who leave the Ecole des Ponts-et-Chaussées are exposed to, that they are obliged to rely greatly upon the conducteurs of that body for all that may relate to the practical details of the pursuit.

When the Ecole des Ponts-et-Chaussées was first instituted, in 1747, by M. Trudaine, on its footing, and Perronet was appointed director of it, the class of education was more general and more decidedly practical than it is now; and the consequence of this was that the productions of the first engineers of the Ponts-et-Chaussées were characterized by much greater taste than those of their successors. It happens in France, and has been observed to take place in England, that the new inventions and the new processes that are introduced into the building trade are all of them of a nature to require the knowledge by those who employ them, of the powers of resistance of materials; and as architects are seldom disposed to study the scientific part of their profession (which would be required in the case of these new processes), it follows that the engineers are always consulted when they are to be employed. The inconvenience of the course followed in the education of young engineers is beginning to be felt in the supreme ugliness that they give to the monuments they are called upon to design; and there are serious questions whether it would not be worth while to change the system of education, in order to introduce some principles of taste, such as had marked the productions of the earlier engineers. Perhaps this might be effected, if the knowledge of free-hand drawing from the figure were made an essential part of the course of examination, and proof were required of the attendance of the pupils at course of lectures in architecture and archæology; but this is at any rate certain, that the engineers of the Ponts-et-Chaussées invariably spoil the opportunities that are afforded them to erect civil buildings, and that the various bridges that they are employed to execute are very deficient in the monumental grandeur that characterized the productions of Huppeau, Pitrou, Perronet, Chezy, Lamblardie, De Prony, &c. This is, after all, a question that interests the lovers of æsthetics, and may for the time be set aside, the more especially as the tendency of everything that is happening in England and America is towards the same state of indifference to the effect of the great work entrusted to the engineers; provided the works satisfy the conditions of stability that are required in them, the public seems contented to set aside the consideration of their beauty in France as well as in England; and the accusation against the engineers of the Ponts-et-Chaussées, that their productions are deficient in taste, must, at any rate, be shared by their colleagues in other countries that pretend to as high a state of civilization as France.

The engineers, however, after leaving the school, are sent, as was said, to the place that may be vacant, whatever the nature of the occupation may be there; they are then promoted, as was before said, in the order of their seniority, unless there be some very strong grounds for making an exception for or against them. They are expected to observe towards one another the kind of respect that is always observed in a military organization, and to exact the same respect from their subordinates; so that the conducteurs are always treated with the same kind of distance with which an officer would treat a common soldier. There are three kinds of service—the service ordinaire, the service extraordinaire, and the service détaché. Of these, the service ordinaire comprehends three classes—the service général, that includes all the ordinary kinds of works that fall within the duties of the Ponts-et-Chaussées; the service spécial, that embraces the direction of the works that are separated from the state budget, and are paid for by the departments; and the service des conseils généraux, that comprehends the general administration of the body, and the service of the Ecole des Ponts-et-Chaussées, and of the depot of maps and plans. The service extraordinaire includes the superintendence of various works that are paid for by the state, but do not enter into the ordinary occupations of the engineers; such as the service of the lighthouses, the works against inundations, the reclamation of the Sologne, &c.; whilst the service détaché comprehends the works that are executed for the departments of the state that are not included in the budget of the Minister of Public Works; such as the harbors of Brest, Cherbourg, L'Orient, Rochefort, Toulon, the colonies, the expeditions to foreign countries, Algeria, and the municipal service of Paris, Lyons, and other large towns. The salaries paid to the various grades are—for the inspecteurs-généraux de 1re classe, 12,000 francs per annum; of the 2nde classe, 10,000 francs per annum; ingénieurs-en-chef de 1re classe, 6000 or 5000 francs per annum; of the 2nde classe, 4500 francs per annum; ingenieurs-ordinaires de 1re classe, 3000 francs per annum; ditto, of 2nde classe 2500 francs per annum; ditto, of 3me classe, 1800 francs per annum; the pupils of the Ecole des Ponts-et-Chaussées receive 1200 francs per annum, and when they are employed on mission they receive 1800 francs. The engineers are, besides their salary, entitled to charge their office expenses, which are fixed by the minister; but, with this exception, and the exception of some works that the engineers are allowed to undertake for the account of the communes, they are strictly limited to the appointments allowed them by the government. I can myself vouch for the fact that the engineers, notwithstanding the moderate amounts of these salaries, are almost universally pure and honest, in the noblest degree; they scorn to receive bribes; and the tone of morals that prevails amongst them in all that relates to pecuniary affairs might serve as a useful lesson to ourselves. This is, perhaps, aided by the conviction that they cannot be discharged from their functions, or debarred from their promotion, unless under very peculiar circumstances of misconduct. They have also a retiring pension in their old age, and their position in society is about the highest in the French administration, from the esteem and respect that always attaches itself to the possession of the class of knowledge that they must attain. The influence of the money-making tendency of the age may be detected, perhaps in some cases, but generally speaking, every one that has had occasion to know the Ingénieurs des Ponts-et-Chaussées must bear the highest testimony to their profound sense of honor and delicacy of feeling.

One great cause of the efficiency of the service of the engineers is, however, to be found in the body of conducteurs or clerks of the works whom they employ, and who are engaged upon the field operations, the superintendence of the workmen, the measurement of the works done, and the preparation of the drawings that are sent out from the engineer's office. The nomination of these men is rather singular, and takes place somewhat after this fashion:—The engineer receives a young man into his office, without salary at first, and he brings him up to the details of his business; at a certain time the men present themselves for examination as conducteurs, and they become embrigadés if they can pass that ordeal, which is made to embrace the whole range of sciences that are concerned with the practice of civil engineering, such as geometry, the theory of numbers, logarithms, the drawing of plans, levelling, taking out quantities, measuring work, the superintendence of it, and generally all that would be required for carrying works into effect. They become then conducteurs embrigadés de 4me classe, and they pass through the ranks of the third, second, and first classes, in the order of their seniority generally; but they may mount in grade more rapidly, in proportion to their merit or their patronage. By the law of 30th October, 1850, it was provided that the sixth of the places of the engineers should be reserved to the conducteurs that had been in the service for the space of ten years, and who could then pass the examination that the engineers were subjected to; but this provision has hitherto been perfectly nugatory, for since its promulgation there has not been a single promotion from the body of conducteurs. The utmost they can aspire to is to be allowed to perform the functions of a civil engineer, whilst they retain their old rank. They would be regarded as intruders if they forced themselves into the society of engineers, as the equals of the latter, by taking advantage of the letter of the law. The consequence of this state of affairs is, that the pupils of the Ecole Polytechnique have practically the monopoly of the government employment in the place of the Ponts-et-Chaussées, for the conducteurs do not aspire to that title, let them be ever so learned or ever so capable; and thus the French revolution has perpetuated the inequality of conditions, by giving the monopoly of the profession to those that can afford to pay for the education that is given in the schools—a strange commentary upon the doctrines of equality that the revolution was intended to inaugurate.

There is a very great inconvenience arising from the strictly theoretical style of education that the young engineers are called upon to go through, that consists in the desire the bulk of them possess to distinguish themselves by the construction of works of a monumental character, without any regard to the conditions of economy that would be applicable in the majority of cases. The principle of the French law is that the architect is responsible for the solidity of the building that he may put up, against any defects of construction or erection, for the period of thirty years; and though the Ingénieurs des Ponts-et-Chaussées do not come under the strict application of the law, owing to their not working for a commission, yet the tendency of the law is to produce a great degree of strength, and a solidity of execution, that may be traced in all the works they execute. The administration requires, in addition, that no work should fail; the engineer that risks much is looked upon as a dangerous and troublesome man. There is every inducement, therefore, for the engineer to erect "monuments," and the character of the works of the Ponts-et-Chaussées is marked with the grandeur and the magnificence that was formerly the distinguishing mark of the Roman engineers; but the cheap and expeditious systems that prevail in England and America are regarded with dislike and contempt. One other objection may be made to the French system, in the total absence of original thought that it developes. The best subjects, as they call them, are obliged every year to travel in foreign countries, and the works that are done in France are generally the productions of civil engineers and the somewhat despised race of architects. The Ingénieurs des Ponts-et-Chaussées do not attempt to produce any new style—they only seek to construct "monuments" that shall last forever regardless of cost. The tendency of the body of the Ponts-et-Chaussées to this style of work is also, to a great extent, assisted by the publication of the journal of the body, that appears about six times a year, under the name of the "*Annales des Ponts-et-Chaussées*," and is characterized by the excessive love of theory, and the abuse (if I may say so much) of mathematical and other scientific knowledge, that may be reproached to the whole of the present school of French engineers. The members of that profession who communicate the information that is inserted in this journal, are, in fact, obliged to treat every subject which they undertake in the most abstract manner, as though they constructed or designed works for the sake of resolving problems of science, instead of endeavoring to answer the questions that might arise, with the purpose of constructing the works economically; they cultivate science as the end of their labors—they do not look upon it as the means to the end. The same observation may be extended to the bulk of French scientific works, whether they be brought out by the Ingénieurs des Ponts-et-Chaussées or by the ingénieurs civils, for the latter are obliged to follow the example set by their rivals, and to employ a great amount of science on the simplest operations of their pursuit. Yet there is a great advantage in thus substituting the indications of theory for the rule-of-thumb processes that are still too prevalent here; and the contrast that is presented between the French engineering journals and the English productions of the same kind is but little to the credit of our scientific and practical men.

(To be Continued.)

To Deaden Sound.

From the London Builder, No. 1107.

In answer to your correspondent as to the best means of deadening sound, when altering an old house in North Lancashire, about four years ago, I met with the best deadened floor I ever saw. Long moss had been gathered in the adjoining woods, dried, and mixed with dry lime rubbish, and filled in between the joists. I adopted the same method in the new portion of the house with equal success. The moss has the advantage of being exceedingly light. The lime rubbish should be well screened and mixed with the moss, great care being taken to leave no part unfilled. The neglect of this is, in my experience, a very common cause of failure. R.

Effect of Curvature in Retarding the Passage of Trains.

From the London Mechanics' Magazine, April, 1864.

American engineers appeared to underrate the effect which curvature produced in retarding the passage of trains; while English authorities differed materially in their views upon the subject. As the author believed practical data were wanting, he had appended to the paper a list containing the length of every curve and straight line upon the Tabon incline, specifying the gradient in each case. Further, he had made a few imperfect experiments, to ascertain the resistances due to sharp curves in combination with steep gradients, the results of which might be taken at all events as approximations to the truth. They were tried with a dynamometer, similar to a spring balance in its form and arrangement, but of only sufficient power to allow of one loaded wagon, of a gross weight of 10 tons, being employed. According to these experiments, the particulars of which were given in detail, upon a straight line with a gradient of 1 in 50, the resistances were 55 ℔s. per ton of train, corresponding with those deduced by the ordinary mode of calculation: while upon the same gradient combined with curves of 600 feet radius, the resistances were augmented to 70 ℔s. per ton of train. By the formula of Latrobe, the resistance due to this curvature was only 5·09 ℔s. per ton of train. Professor Rankine's gave 12·3 ℔s. per ton of load. These experiments appeared to show that, upon a curve of 1000 feet radius, the resistance was double that upon a straight line; and upon a curve of 600 feet radius, it was equal to the gravity on an inclined plane of 1 in 150. Approximately, the resistance due to the curvature might be found, either by dividing the number of lbs. in a ton by 0·224 of the radius of the curve, or by multiplying the deflection angle of the curve by 1·75, to give the resistance in lbs. per ton of train. It should be stated, that the gauge of the line was 5 ft. 6 ins. and that the proper super-elevation had been given to the outer rail on curves.

For the Journal of the Franklin Institute.

Solutions of a Problem of the Rafters. By S. W. Robinson, C. E., Detroit, Mich.

I offer the following for publication for the interest manifested in consequence of the numerous solutions by the "triangle of forces," by "moments," by "virtual velocities," or by "the shearing stress." I have met with scarcely another problem whose solution by either of these principles can be made with so nearly equal facility.

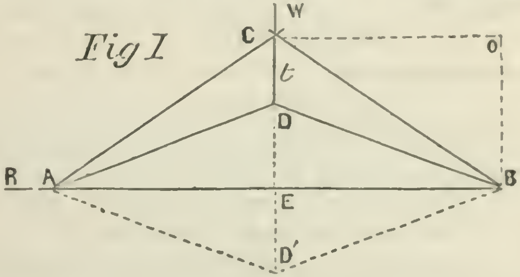


Fig 1

Let AC, CB represent the rafters AD, DB or AD′, D′B sub-rafters, or rods joined at their intersection D or D′ with the intersection at C by a tie or strut DC or D′C. And let W be a force acting at C tending to depress C, causing a thrust at A and B, tending to straighten AD, DB or AD′, D′B causing a stress upon DC, or D′C.

Required the amount of stress t upon DC, *or* D′C.

When the force W is acting at C, and the combination is in equilibrium, there results a force t acting at C. There is a horizontal thrust at A and B caused by the force at C, which is resisted by an equal and opposite force in AD, and DB. Call this force R.

Now as the forces in any triangle of forces are proportional to the sides of the triangle respectively, we will have from the triangle

AEC, $\mathrm{AE} : \mathrm{EC} = \mathrm{R} : (\mathrm{W} \pm t)$ whence $\mathrm{R} = (\mathrm{W} \pm t)\frac{\mathrm{AE}}{\mathrm{EC}}$.

Also from the triangle AED, $\mathrm{AE} : \mathrm{ED} = \mathrm{R} : t$, whence

$$\mathrm{R} = \frac{\mathrm{AE}}{\mathrm{ED}}\,t \text{ or } \frac{\mathrm{AE}}{\mathrm{ED'}}\,t = (\mathrm{W} \pm t)\frac{\mathrm{AE}}{\mathrm{EC}}, = \mathrm{W}\frac{\mathrm{AE}}{\mathrm{EC}} \pm t\,\frac{\mathrm{AE}}{\mathrm{EC}}$$

by equating; from which

$$t\left(\frac{1}{\text{ED or ED}'} \mp \frac{1}{\text{EC}}\right)=\frac{\text{W}}{\text{EC}};$$

$$\text{whence } \frac{t}{\text{EC}}\left(\text{EC} \mp \text{ED or ED}'\right)=\frac{t}{\text{EC}}\left(\text{CD, or CD}'\right)=\frac{\text{W}}{\text{EC}}(\text{ED or ED}')$$

$$\therefore\ t=\text{W}\frac{\text{ED}}{\text{CD}} \text{ or W}\frac{\text{ED}'}{\text{CD}'}.$$

To make the solution by moments, draw CO and OB parallel to EB and EC.

$$\text{Then} \qquad \text{R}\,.\,(\text{BO}=\text{EC})=(\text{W} \pm t)(\text{CO}=\text{EB})$$

$$\text{and} \qquad t\,.\,\text{EB}=\text{R}\,.\,(\text{ED or ED}')$$

$$\text{or} \qquad t\,.\,\text{EB}=(\text{W}+t)\frac{\text{EB}\,.\,\text{ED}}{\text{EC}}$$

$$\text{from which} \qquad t=\text{W}\,\frac{\text{ED}}{\text{CD}}.$$

To make the solution by the principle of virtual velocities, we have from the triangle AEC

$$\overline{\text{AE}}^2+\overline{\text{EC}}^2=\overline{\text{AC}}^2,$$

And from the triangle AED

$$\overline{\text{AE}}^2+\overline{\text{ED}}^2=\overline{\text{AD}}^2.$$

Now if C be depressed through an element of space, A will move horizontally through an element of space in which case AE, EC, and ED become variable, while AC and AD remain constant. Therefore we may differentiate the two equations above and obtain

$$2\,\text{AE}d(\text{AE})+2\,\text{EC}d(\text{EC})=0 \text{ and } 2\,\text{AE}d(\text{AE})+2\,\text{ED}d(\text{ED})=0.$$

Eliminating $\text{AE}d(\text{AE})$ gives

$$\frac{d(\text{EC})}{d(\text{ED})}=\frac{\text{ED}}{\text{EC}}.$$

The element of space $d(\text{EC})$ may be regarded as that through which C passes; and $d(\text{ED})$ the element through which D passes. Hence each force multiplied into its displacement gives

$$(\text{W}+t)\,.\,d(\text{EC})=t\,.\,d(\text{ED}),$$

$$\text{or} \qquad (\text{W}+t)\frac{d(\text{EC})}{d(\text{ED})}=t.$$

Substituting for the ratio of the infinitessimals its value above, then

$$(\text{W}+t)\frac{\text{ED}}{\text{EC}}=t,$$

$$\text{from which} \qquad t=\text{W}\frac{\text{ED}}{\text{CD}}.$$

To make the solution be means of the principle of shearing stress. The shearing stress of a beam with reference to any section, is the amount of force which tends to move one part of the beam over the other, every particle of the beam moving in a direction parallel to this section.

Therefore if the force in AC be P and that in AD be P′, the vertical shearing stress of AC will be

P sin EAC; and as this is the total vertical resistance of AC; it must be equal to the total vertical force acting upon it;

Hence $$\mathrm{P}\sin\mathrm{EAC}=\mathrm{W}+t.$$

Similarly the vertical shearing of AD is
$$\mathrm{P}'\sin\mathrm{EAD}=t.$$

Taking the horizontal shearing we have
$$\mathrm{P}\cos\mathrm{EAC}=\mathrm{R},\text{ and }\mathrm{P}'\cos\mathrm{EAD}=\mathrm{R}.$$

Reducing we get
$$\mathrm{R}\ \mathrm{tang}\ \mathrm{EAC}=\mathrm{W}+t,\text{ and }\mathrm{R}\ \mathrm{tang}\ \mathrm{EAD}=t,$$

or $$t\,\frac{\mathrm{tang}\ \mathrm{EAC}}{\mathrm{tang}\ \mathrm{EAD}}=\mathrm{W}+t.$$

But $\mathrm{tang}\ \mathrm{EAC}=\frac{\mathrm{EC}}{\mathrm{EA}}$, and $\mathrm{tang}\ \mathrm{EAD}=\frac{\mathrm{ED}}{\mathrm{EA}}$.

$$\therefore\qquad t\,\frac{\mathrm{EC}}{\mathrm{ED}}=\mathrm{W}+t,\text{ or }t=\mathrm{W}\,\frac{\mathrm{ED}}{\mathrm{CD}}.$$

Other solutions might be made by some of these four principles. Thus, by the triangle of forces, taking AE to represent R, and EC to represent (W+t), and ED to represent t, we would obtain
$$\mathrm{R}\ \mathrm{tang}\ \mathrm{EAC}=(\mathrm{W}+t),\text{ and }\mathrm{E}\ \mathrm{tang}\ \mathrm{EAD}=t,$$
which is reduced similarly as the last.

To make a second solution by the principle of "virtual velocities," let C move through an element of space to C′ Fig. 2. At the same time B will move to B′. Draw through B′, B′H parallel and equal to BC′, then HC=B′B and the triangle HCC′ is similar to CEB, which gives BE : EC=C′C : CH=C′C : BB′

whence $\frac{\mathrm{BB}'}{\mathrm{CC}'}=\frac{\mathrm{EC}}{\mathrm{BE}}$.

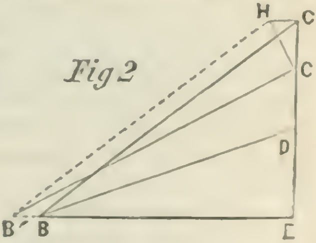


Fig 2

By the principle of "virtual velocities,"
$$\mathrm{R}\ .\ \mathrm{BB}'=(\mathrm{W}+t)\ .\ \mathrm{CC}',$$

or $$\mathrm{R}\,\frac{\mathrm{EC}}{\mathrm{BE}}=\mathrm{W}+t.$$

Similarly we would get from BED
$$\mathrm{R}\,\frac{\mathrm{ED}}{\mathrm{BE}}=t,$$
which may be reduced as similar expressions above.

If in the result for t we make $ED=0$, $t=0$. If we make $CD=0$, $t=\infty$.

If we multiply through by CD and make $CD=\infty$, then $ED=\infty$. Similar conclusions would be drawn from the figure.

If AE and EB, Fig. 1, be taken unequal, we get a similar expression for t.

MECHANICS, PHYSICS, AND CHEMISTRY.

The Macrograph. By Wm. B. Morgan, M. A., Prof. of Math. and Ast. in Earlham College, Richmond, Ind.

To the Editor of the Journal of the Franklin Institute.

The following article is a copy of a thesis which was prepared by Mr. Morgan previous to his taking the degree of Civil Engineer at the University of Michigan, in June, 1863.

I offer it to you for publication on account of its originality, and the hope that it will induce further investigation. It may result in reducing the principles involved to some useful application.

D. V. Wood, Prof. Civ. Eng.

This instrument, being as yet without a name, the author of the following article has concluded to call it *The Macrograph*, from its property of describing long curves. (Gr. μακρος, *long*, and γραφω, *to write.*)

The Macrograph is the invention of Oliver Butler, Attorney at Law, Richmond, Indiana.

I. The instrument is represented by Fig. 1, Plate I. It has two cog-wheels, A and B—the latter moving the former—the *number* of teeth in the two being different.

Each wheel carries an arm, M M′, which may be shortened or lengthened at pleasure by means of adjusting screws. At the extremity of the arm, M, is a rotating pivot, P, with a binding screw and a mortise through which the rod R passes. The rod R is attached to the extremity of M′, and also passes through pivot P′, which may be placed any where upon it at pleasure, and fixed there by means of the binding screw. The pivot P′ rotates in the slider S. The latter may be placed any where upon the rod R. This latter rod has holes in it at H H′, &c., into which a pencil may be inserted.

Upon turning the crank of wheel B, while the instrument is in the position indicated, it will describe a curve or rather a series of curves, of which Fig. 2 is a specimen. By placing the pencil in other parts of the same, four figures may be made which differ so much from this specimen, that one would hardly suppose that they were governed by the same law.

It is evident from the construction of the instrument, that if the wheels contained the same number of cogs in each, with any particular arrangement of the pencil, rods, pivots, &c., the pencil must follow in the same path at successive revolutions of the wheels. This path *might be* a straight line, a loop, or some other shaped figure. While the number of cogs is unequal, it is evident that the path must vary continually until the same relative position of the cogs is reached which existed at the commencement of the curve.

II. The rod R′ may be removed, and the pivot at L inserted into slide S. The mortises in slide S and pivot P have slits in them equal to the thickness of the pencil through which the latter may work. Since the wheels are unequal in size, it is evident that, in order to manipulate the instrument, the binding screw of either the slider or the pivot must be loosened. By this arrangement we may describe two systems of curves differing somewhat in principle of construction.

Fig. 3 is a sample of the curves described when the binding screw of the slider is loosened.

If the binding screw of the pivot be loosened, we may describe curves of which Fig. 4 is a sample.

III. Lastly, the wheel A may be removed, and a pivot in the lower part of the slider S′ inserted into the slider S. The binding screws of S′ and S being tightened, and that of pivot P loosened, a pencil inserted into any of the holes will describe an arc of a circle. If the slider S′ is loosened and pivot P tightened, the pencil immediately under S′ will describe a straight line; at every other point an oval, with its extremity nearer to S′, more or less sharp, according to its proximity to S′.

Figs. 5 and 6 are examples.

Again S′ may be fastened at any point of rod R′, the pivot P being tightened, and the slider S loosened. Fig. 7 shows three such curves described, with the pencil at three different points.

It is this class of curves that I have partially investigated. The discussion of the curves described by the first two arrangements of the instrument, would probably be more curious than practically useful. The only useful application of the first two combinations which suggests itself to the writer, belongs to the engraver's art. A graver's tool, properly constructed upon the same principle, might produce curves which it would be impossible to counterfeit. By the use of cams, eccentrics, and elliptic wheels, not exhibited in the drawing, the figures may be infinitely more diversified.

It is possible that the *single* curves described by the *third* arrangement may be useful to the engineer. At any rate, it is his province to investigate every manner of curve, and ascertain whether in his extensive department for applied mathematics, each may not find some place for conducing to strength and symmetry.

The problem which I have partially solved may be thus stated:

The end of a straight line is made to follow the circumference of a circle, while the line passes through any fixed point: what is the equation and some of the properties of the curve described by any point of the given line?

Let E, Fig. 8, be the centre of the given circle, A B the given line, and p the given point.

Let R = radius of the given circle.
a = length of $AB = A'B'$.
$r = PB = P'B'$ = radius vector of curve.
$d = PE$.
t = angle BPK.

We have,

$$r = a - AP \text{ (or } A'P') = a \pm AF - PF, \qquad \text{(A)}$$

But $PF = d \cos t$,
and from triangle PEA, $d : R :: \sin A : \sin t$;

$$\therefore R \sin A = d \sin t, \qquad (a)$$

Subtract the square of both members of a from R^2, and we find

$$R \cos A = \pm \sqrt{R^2 - d^2 \sin^2 t} = AF \text{ or } A'F, \qquad (b)$$

These in (A) give

$$r = a \pm \sqrt{R^2 - d^2 \sin^2 t} - d \cos t,$$

which is the polar equation of the curve.

From this equation we see that for every value of t there are two values for r, the difference of which is $2\sqrt{R^2 - d^2 \sin^2 t} = 2 R \cos A$ (see (b)). Whence it appears, as is also evident from the construction, that the chord BB' = the chord AA'.

From (a) we have $\sin t = \dfrac{R \sin A}{d}$, from which we see that the superior limit of t is

$$\sin^{-1} \frac{R}{d}, \qquad (c)$$

$$\text{or,} \qquad \cos^{-1} \sqrt{\frac{d^2 - R^2}{d^2}}$$

$\therefore$ if $d = R$ the limit is 90°.

On condition that $d = R$, or that the point through which the given line passes is situated on the circumference of the given circle, the equation of the curve becomes $r = a$ or $r = a - 2 R \cos t$.

With the former the point describes a semicircle, and with the latter a curve meeting the extremities of this semicircle, modified by the length of a.

Fig. 9 shows two such curves. The first differential co-efficient of the latter equation is $\dfrac{dr}{dt} = 2 R \sin t$; $\therefore \dfrac{dt}{dr} = \dfrac{1}{2 R \sin t}$, which is the value of the subnormal. Hence the value of the subnormal is independent of the radius vector. Then, for the same value of t in a system of such curves described with different radii, if normals be drawn to the curves at the extremity of the radii, as at L, L', &c., these normals will all meet in a single point C, PC being the subnormal.

For every radius vector drawn from P through the curves, there would be a particular point, C, where the normals would meet. The *locus* of these points would not be the straight line PC, but would form a curve.

[If several lines of intrenchments be made, one behind the other, and of the form of these curves, then batteries placed in the same radius vector, as at L, L′, &c., might concentrate their fire at the same point, C, when their line of sight is normal to the curve.]

From the theory of curves we have

$$\text{the subtangent}=\frac{r^2dt}{dr}=\frac{(a-2\text{R}\cos t)^2}{2\text{R}\sin t}\quad . \qquad . \qquad (\text{D})$$

$$\text{Tangent}=r\sqrt{1+\frac{r^2dt^2}{dr^2}}=\frac{a-2\text{R}\cos t}{2\text{R}\sin t}\sqrt{4\text{R}^2+a^2-4a\text{R}\cos t}\qquad (\text{E})$$

$$\text{Normal}=\sqrt{r^2+\frac{dt^2}{dr^2}}=\sqrt{(a-2\text{R}\cos t)^2+\frac{1}{4\text{R}\sin^2 t}}\quad . \qquad (\text{F})$$

If in these we suppose $a=2\text{R}$, corresponding to the curve MMP we have

$$\text{Subtangent}=2\text{R}\frac{(1-\cos t)^2}{\sin t},\ \text{for } t=90^\circ=2\text{R}\quad . \qquad . \qquad (d)$$

$$\text{Tangent}\quad=\text{R}\sqrt{8}\frac{(1-\cos t)^{\frac{3}{2}}}{\sin t},\ \text{for } t=90^\circ=\sqrt{8}\text{R}\quad . \qquad . \qquad (e)$$

$$\text{Normal}\quad=\sqrt{\frac{1}{4\text{R}^2\sin^2 t}+4\text{R}^2(1-\cos t)^2},\ \text{for } t=90^\circ=\sqrt{\frac{1+16\text{R}^4}{4\text{R}^2}}\ (f)$$

and if $t=0$ we have subtang $=\frac{0}{0}$, tan $=\frac{0}{0}$, normal $=\propto$, which indicates a cusp.

If in (D), (E), and (F), we make $t=90^\circ$, we shall have

subtan $=\frac{a^2}{\text{R}^4}$, tan $=\frac{a}{2\text{R}}\sqrt{\frac{4a^2\text{R}^2+a^4}{4\text{R}^2}}$, normal $=\sqrt{\frac{1+4a^4\text{R}^2}{4\text{R}^2}}$ and if $t=0$,

subtan $=\frac{(a-2\text{R})^2}{0}=\propto$, tan $=\propto$, normal $=\propto$.

To find the area, use the equation $d\text{A}=\frac{1}{2}r^2dt$, in which substitute the value of $r=a-2\text{R}\cos t$, and integrate, and we readily find

$$\text{A}=\tfrac{1}{2}a^2t-2a\text{R}\sin t+\text{R}^2t+\tfrac{1}{2}\text{R}^2\sin 2t+(\text{C}=0).$$

For $t=90^\circ$ we have

$$\text{A}=\tfrac{1}{4}\pi a^2-2a\text{R}+\tfrac{1}{2}\pi\text{R}^2$$

Hence the whole area N N N $=\pi(\frac{1}{2}a^2+\text{R})-4a\text{R}$.

If $a=2\text{R}$ we find for the area M M P M M $=(3\pi-8)\text{R}^2$.

Radius of Curvature.

In the general expression

$$\rho=\frac{\left[r^2+\left(\frac{dr}{dt}\right)^2\right]^{\frac{3}{2}}}{r^2+2\left(\frac{dr}{dt}\right)^2-r\frac{d^2r}{dt^2}},$$

Substitute $r=a-2\text{R}\cos t$, $\frac{dr}{dt}=2\text{R}\sin t$, $\frac{d^2r}{dt^2}=2\text{R}\cos t$ and reduce, and we find

$$\rho=\frac{[a^2-4a\text{R}\cos t+4\text{R}^2]^{\frac{3}{2}}}{a^2-6a\text{R}\cos t+8\text{R}^2} \quad . \quad . \quad . \quad \text{(G)}$$

If $t=0$, then $\rho=\rho_0=\frac{(a-2\text{R})}{a^2-6a\text{R}+8\text{R}^2}$ . . . (G′)

If $t=90°$ then $\rho=\rho_1=\frac{(a^2+4\text{R}^2)^{\frac{3}{2}}}{a^2+8\text{R}^2}$ . . . (G″)

In these make $a=2\text{R}$ and we find

$$\rho=\frac{2^{\frac{5}{2}}}{3}\text{R}(1-\cos t)^{\frac{1}{2}} \quad . \quad . \quad . \quad \text{(H)}$$

$$\rho_0=\tfrac{0}{0} \quad . \quad . \quad . \quad . \quad \text{(H′)}$$

$$\rho_1=\frac{2^{\frac{5}{2}}}{3}\text{R} \quad . \quad . \quad . \quad . \quad \text{(H″)}$$

For T, T′ we have $\text{TT}'=r\cos t=a\cos t-2\text{R}\cos^2 t$,

which is a maximum for $\cos t=\frac{a}{4\text{R}}$ . . . . (I)

For $a=2\text{R}$, $\cos t=\frac{1}{2}$ $\therefore$ $t=60°$

Application to the Arch.

Suppose we have an arch constructed with the curvature NNNN, having a pier in the centre. This would make a graceful arch.

Let θ= the angle which the tangent line to the curve makes with NPN, or with the axis of x, and

V= the angle which the tangent line makes with the radius vector at the point of tangency.

Then $\tan \text{V}=r\frac{dt}{dr}=\frac{a-2\text{R}\cos t}{2\text{R}\sin t}$

$$\theta=\text{V}-(90-t)=\text{V}+t-90°$$

$$\therefore \tan\theta=-\cot(\text{V}+t)=\frac{\tan\text{V}\tan t-1}{\tan\text{V}+\tan t}=\frac{a\tan t-4\text{R}\sin t}{a-2\text{R}\,(\cos t-\sin t\tan t)}.$$

Substitute $\frac{\sin t}{\cos t}$ for $\tan t$ and reduce, and we have

$$\therefore \operatorname{Sec}^3\theta=\left[1+\left(\frac{a\sin t-2\text{R}\sin 2t}{a\cos t-2\text{R}\cos 2t}\right)^2\right]^{\frac{3}{2}} \quad . \quad . \quad \text{(K)}$$

Now if z=depth of loading at any point,
m=depth of loading at the crown, N,
ρ_0=radius of curvature at the crown,
ρ=radius of curvature at the point corresponding to z, we have (See Professor Wood's Lectures)

$$z=\frac{m\rho_0}{\rho}\sec^3\theta \quad . \quad . \quad . \quad \text{(L)}$$

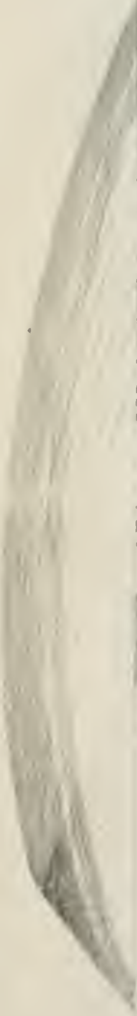


Fig. 1.

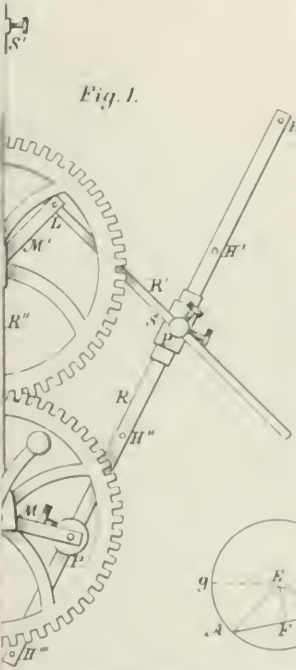


Fig. 3.

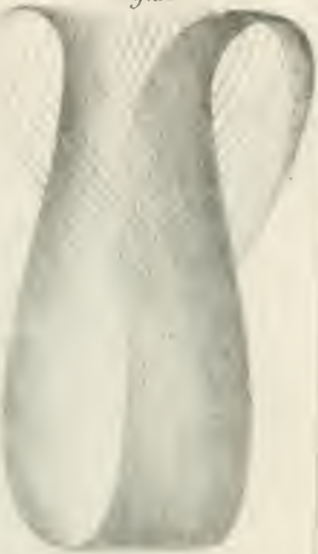
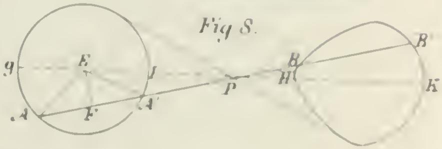


Fig 8.



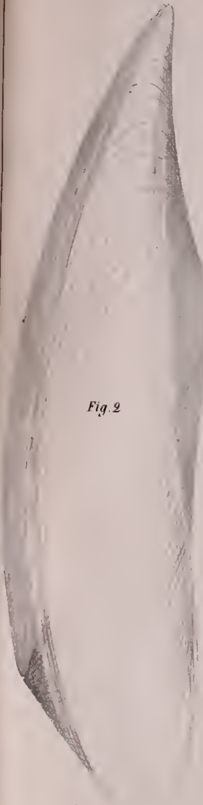


Fig. 2

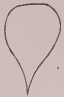


Fig. 5.

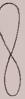


Fig. 6.

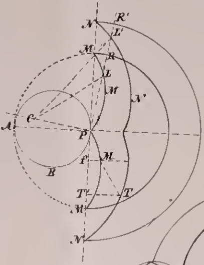


Fig. 9.

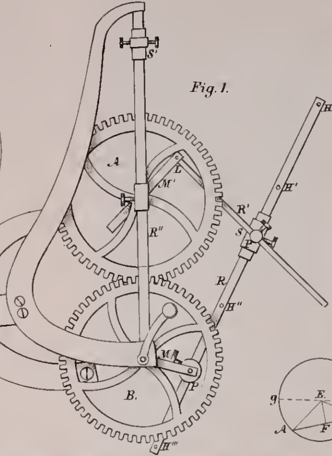


Fig. 1.

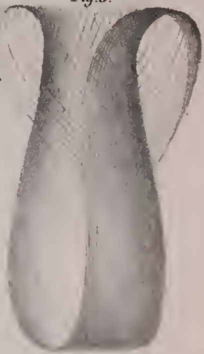


Fig. 3.

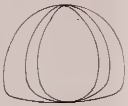


Fig. 7.

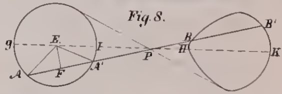


Fig. 8.

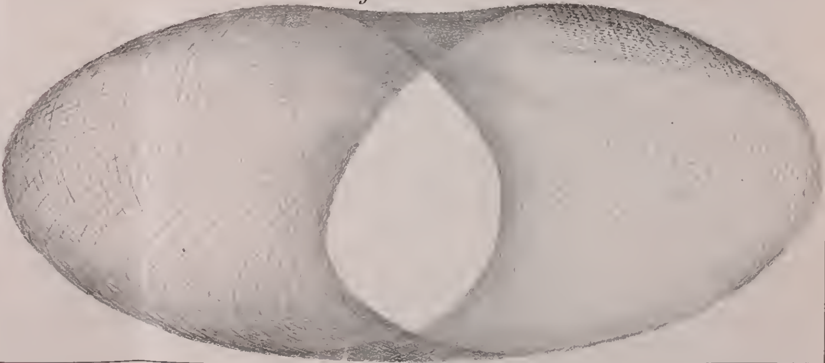


Fig. 4.

Equation (L) is thus deduced—

If the arch is a curve of equilibrium, the resultant of the forces at any point will be in the direction of the tangent.

Let T=the tension or compression at any point,
$\Sigma\mathrm{Y}$=the sum of all the vertical forces from the crown to the point considered,
H=the thrust at the crown,
δ=the weight of a unit of volume.

The other notation is given above.

Resolving the tension horizontally and vertically, we have

$$\mathrm{T}\frac{dx}{ds}=\Sigma\mathrm{Y}=\int\delta z\,dx.$$

$$\mathrm{T}\frac{dx}{ds}=\mathrm{H}.$$

Dividing gives

$$\frac{dy}{dx}=\frac{\int\delta z\,dx}{\mathrm{H}}=\tan\theta.$$

Differentiating gives $\dfrac{d^2y}{dx^2}=\dfrac{\delta z}{\mathrm{H}}$.

These in the value of the radius of curvature give

$$\rho=\frac{\left(1+\frac{dy^2}{dx^2}\right)^{\frac{3}{2}}}{\frac{d^2y}{dx^2}}=\mathrm{H}\frac{\sec^3\theta}{\delta z}$$

$$\therefore\quad z=\frac{\mathrm{H}\sec^3\theta}{\delta\rho}.$$

For $\theta=0$, $z=m$, and $\rho=\rho_\circ$ $\therefore$ $m=\dfrac{\mathrm{H}}{\delta\rho_\circ}$ which combined with the former equation gives

$$z=\frac{m\rho_\circ\sec^3\theta}{\rho}\text{ as given above.}$$

To find $\rho_\circ$ substitute $\cos t=\dfrac{a}{4\mathrm{R}}$. (See (I)) in (G) and we find

$$\rho_\circ=\frac{16\mathrm{R}^3}{16\mathrm{R}^2-a^2}.\qquad\qquad (\mathrm{M})$$

Substitute the value of $\sec^3\theta$ from (K), ρ from (G) and $\rho_\circ$, in (L) and we find

$$z=m\frac{16\mathrm{R}^3}{16\mathrm{R}^2-a^2}\left[\frac{a^2-6a\mathrm{R}\cos t+8\mathrm{R}^2}{(a^2-4a\mathrm{R}\cos t+4\mathrm{R}^2)^{\frac{3}{2}}}\right]\times$$
$$\left[1+\left(\frac{a\sin t-2\mathrm{R}\sin 2t}{a\cos t-2\mathrm{R}\cos 2t}\right)^2\right]^{\frac{3}{2}}\qquad .\qquad (\mathrm{M}')$$

If $\cos t = \frac{a}{4\text{R}}$ we have $z = m$

$$t = 90^\circ \text{ we have } z = 2m\,\frac{8\text{R}^2 + a^2}{16\text{R}^2 - a^2} \quad . \quad . \qquad (\text{M}'')$$

$$t = 0 \quad \text{we have } z = m\,\frac{16\text{R}^3}{4\text{R} + a} \cdot \frac{(4\text{R} - a)^{\frac{1}{2}}}{(2\text{R} - a)^{\frac{3}{2}}} \quad . \qquad (\text{M}''')$$

If $a = 2\text{R}$, which corresponds to the curve MMP we have

$$z = \tfrac{1}{2}m\,\frac{1}{\sin\frac{1}{2}t}\left[1 + \left(\frac{\sin t - \sin 2t}{\cos t - \cos 2t}\right)^2\right]^{\frac{3}{2}} \quad . \qquad (\text{N})$$

$$\text{for } t = 60^\circ,\ z = m \quad . \quad . \quad . \quad . \qquad (\text{N}')$$

$$t = 90^\circ,\ z = 2m \quad . \quad . \quad . \quad . \qquad (\text{N}'')$$

$$t = 0,\quad z = \infty \quad . \quad . \quad . \quad . \qquad (\text{N}''')$$

From (N″) we see that the depth of loading at the extremity should be twice that at the crown.

If, then, a portion contained between $t = 60^\circ$ and $t = 90^\circ$ were taken for half the span of an arch, and the other half span constructed similarly, we should have the form of an arch which might, in some cases, be useful, for instance, in covered arches for aqueducts. The upper portion of the load may be horizontal, provided its thickness over the top of the arch is equal to the rise of the arch.

Utilization of Brine. A Practical Application of Dialysis.

From the London Chemical News, No. 225.

Mr. Whitelaw, of Glasgow, has recently described the result of a patent process of his own for utilizing the brine of salted meat. When fresh meat, he said, had been sprinkled with salt for a few days, it was found swimming in brine. Fresh meat contained more than three-fourths of its weight of water, which was retained in it as in a sponge. But flesh had not the power to retain brine to that extent, and in similar circumstances it absorbed only about half as much saturated brine as of water, so that under the action of salt flesh allowed a portion of its water to flow out. This expelled water, as might naturally be expected, was saturated with the soluble nutritive ingredients of flesh—it was, in fact, juice of flesh—soup—with all its valuable and restorative properties. In the large curing establishments of this city very considerable quantities of this brine were produced, and thrown away as useless. This was the material to which Mr. Whitelaw has applied the process of dialysis, and he thought with success, for the removal of the salts of the brine, and for the production at a cheap rate of pure fresh extract of meat. His process he stated as follows:—The brine, after being filtered to free it from any particles of flesh or other mechanical impurities it might contain, was then subjected to the operation of dialysis. The vessels or bags in which he conducted the operations might be made of various materials and of many shapes, but whatever might be their material or shape he called them "dialysers." Such an apparatus as the following would be found to answer the purpose:—A square vat made of a frame work of iron filled up with sheets of skin or parchment paper in such a way as to be water-tight, and strengthened if necessary by stays or straps of metal. The sides, ends, and bottom being composed of this soft, dialysing material, exposed a great surface to the action of the water contained in an outer vat, in which the dialyser was placed. He found a series of ox bladders fitted with stop-cocks, or gutta percha mouth tubes, and plugs, and hung on rods stretching across and into vats of water, a very cheap and effective arrangement. He could also employ skins of animals, either as open bags or closed, and fitted with stop-cock's, or bags of double cloth, with a layer of soft gelatine interspersed between them. Other arrangements would readily suggest themselves, and might be adopted according to circumstances. But supposing the bladder arrangement were taken, which, he thought, would be found practically the best, being cheap, easily managed, and exposing a great surface to the dialytic action. The bladders were filled with the filtered brine by means of fillers, and hung in rows on poles across, and suspended into vats of water. The water in those vats was renewed once a day, or oftener if required, and he found that actually at the end of the third or fourth day, according to the size of the bladders employed, almost all the common salt and nitre of the brine had been removed, and that the liquid contained in the bladders was pure juice of flesh in a fresh and wholesome condition. The juice as obtained from the "dialysers," might now be employed in making rich soups without any further preparation, or it might be concentrated by evaporation to the state of solid extract of meat. The liquid from the dialysers might be treated in several ways. It might be evaporated in an enamelled vessel to a more or less concentrated state, or to dryness, and in these various conditions packed in tins or jars for sale. It might be concentrated at a temperature of 120°, by means of a vacuum-pan or other suitable contrivance, so as to retain the albumen and other matters in a soluble form. Again, the more or less concentrated liquid might be used along with flour used in the manufacture of meat biscuits. The products he had named were all highly nutritive, portable, and admirably adapted for the use of hospitals, for an army in the field, and for ships' stores. The dialysis of brine might be conducted in salt water, so as to remove the greater portion of its salt, and the process completed in a small quantity of fresh rain or other water. In this way ships at sea might economize their brine, and so restore to the meat in a great measure the nutritive power that it had lost in the process of salting. Thus, then, Mr. Whitelaw obtained an extract of flesh at a cheap rate, from a hitherto waste material. Two gallons of brine yielded one pound of solid extract, containing the coagulated albumen and coloring matter. For the production of the same directly from meat, something like twenty pounds of lean beef would be required. The quantity of brine annually wasted was very great. He believed he was considerably under the truth when he said that in Glasgow alone 60,000 gallons were thrown away yearly. If they estimated one gallon as equal to seven pounds of meat in soup producing-power, then this was equal to a yearly waste of 187 tons of meat without bone. Estimating the meat as worth six-pence per pound, this amounted to a loss of £10,472. In this way the waste over the country must be very great. In the great American curing establishments the brine wasted must be something enormous, as he found that in eight of the Federal States 4,000,000 pigs were slaughtered and cured last season. Mr. Whitelaw concluded by quoting from Gregory and Liebig as to the value and efficacy of extract of meat.

New mode of Illumination.

From the London Chemical News, No. 225.

M. Soubra, a Professor of Mathematics, has invented a new method of illumination, or rather a method of inverting a flame by the adoption of which several advantages are expected to be realized. The apparatus consists of a syphon of glass, the open ends of which are turned upwards; a burner is placed just within the shorter branch. Before lighting the lamp, the longer branch of the syphon, is heated and a current of air established, which carries with it any flame placed at the open end of the shorter branch, the flame, consequently becoming inverted. As soon as the current is established, and the burner kindled, the heating of the longer leg may be discontinued, the current, once established, being sustained by the heat from the inverted flame. The advantages of this new arrangement are as follows:—The supports of the globes or lamp-glasses are placed above the flame, and do not intercept the light; the reflectors, also are in no danger of becoming blackened by smoke, and they collect rays that otherwise would be lost in the air. The flame has a more elevated temperature on account of the heat being concentrated by the syphon, and the carbon consequently more incandescent. The products of combustion are collected in the syphon, and may be conveyed away instead of vitiating the air of the apartment. It is proposed to employ these reversed flames as foot-lights for theatres, the advantages, such as safety, &c., being obvious.

Translated for the Journal of the Franklin Institute.

Water-proof Mortar.

M. Pavesi of Mortara, recommends for the protection of houses against dampness, to cover the walls with a plaster composed as follows: 4 parts of pounded glass, 3 parts of charcoal or coal, 2 parts of pomice-stone, 3 parts of tar, 2 parts of pitch, 1 part of boiled linseed oil. These substances are mixed in a cast iron pot, and moderately heated until they form a homogeneous paste. It is then to be applied to the wall and sprinkled with sand for the purpose of holding the lime which is to be applied over it. This mortar is recommended by its efficiency, cheapness, and easy preparation.—*Cosmos.*

Of the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, No. 196.

1. *The Pneumatics of the Ancients.*—In the earliest ages of which we have any record the idea that the four elements—earth, water, air, and fire—were first created, and the whole world produced from them, seems to have prevailed. We conceive this to have been the oldest physical theory; it was believed by different races in various parts of the world; indeed, it is probable that it originated before the time of the Arian migration. Races which differed from each other in every respect in which they could differ—in habits, in power of thought, in religion—all admitted this theory. The philosophy of every age is pervaded by it, and it endured longer than any succeeding hypothesis; for a century ago it was not as yet disproved.

We can quite understand how this thought came to fix itself so early and so permanently in the human mind. In every age, in every country, we find some men who possess a spirit of inquiry, who desire to understand all they can about the world in which they live, and to account for the various phenomena they see around them. In this same spirit did the first philosopher ask himself, "What is the world?" Throughout Nature he found but four distinct entities, and was it not natural to think the whole world was composed of them alone? Gross earth, to which he could refer all solids; more mobile water; scarcely perceptible air, with which he classed winds, and clouds, and his own breath; and lastly, subtile, intangible fire, type and symbol of the sun which he worshipped, first as an emblem of the Deity, but afterwards as himself a god.

In several systems of philosophy the air was made chief of the elements; from it the other three proceeded, and from all four the world. Kanáda, who founded the Nyaya system of Hindu philosophy, at a period long prior to the rise of Greek physical thought, taught that all material substances exist first as atoms, then as aggregates. "When atoms are separated, dissolution of the world takes place; at its renovation the atoms come together again to form air, then fire, next water, afterwards earth, then the body and its organs, and ultimately the whole world."* The distinguishing feature of air, he tells us, is its being neither hot nor cold; it possesses, moreover, "faculty of elasticity and velocity."

In each of the three ancient schools of Hindu philosophy a fifth element, the "ether occupying space," was introduced; it was supposed to be the vehicle of sound, which some said was propagated by waves. It is difficult to understand in what light the ether was viewed by the San'chya and Nyaya schools, but according to the Vedanta (a system which followed that of Kanáda), the ether was air at rest, and the "air" wind or air in motion. Ether and air, we are told, were first created by Brahme, and fire, water, and earth produced in succession, the one from the other. Thus air would be evolved from ether, and fire from air.

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* Colebrook "Philosophy of the Hindus."

In both the Nyaya and Vedanta systems fire is said to be produced from air, and this idea probably arose from the knowledge of the necessity of air to fire. The first fire was in all probability produced by the friction of two pieces of wood, for this method is mentioned in the earliest hymns of the Rig Veda, the oldest book in the world next to the Pentateuch. The first man who produced fire by friction must have observed how the ignited wood glowed when he moved it quickly through the air, or when the air blew upon it; and, in order to convert the first produced spark into a flame, he would in future blow upon it, for since wind increased the energy of fire, it seemed likely that the breath, which felt like wind, would also increase it; and soon a reed tipped with metal would be used to concentrate the breath on one spot, in cases where great heat was required. We find such reeds were used by the Egyptian workers in metal as early as the time of Abraham;* they were afterwards superseded by pointed metal tubes. But the use of a mouth blow-pipe was laborious, and it could not be used for large fires or smelting operations; hence men set about finding something which should take the place of the lungs, and in their search they were probably helped by the makers of leather bottles; for these latter could not fail to observe that when a bottle was squeezed a blast of air rushed from its mouth, and by making a hole at the opposite end to the mouth, closing this when the bottle was compressed, and opening it to admit the air before a second compression, a tolerably continuous blast could be produced. Hence arose the invention of bellows. These primitive bellows seem to have been improved at a very early date by the Egyptians, for Sir Gardiner Wilkinson believes he has found indications of a valve by which air was admitted to the bellows as early as the time of Moses.

Let us next turn our attention to the ideas of some of the Greek philosophers in reference to the air.

Anaximenes, of Miletus (born B.C. 529), taught that the air was the beginning of things, from which every thing had been produced, and into which every thing would be resolved.† The earth was a trapezium supported by the air.

Air when rarefied becomes fire; when condensed, winds and clouds; more condensed, water; and condensed to its greatest extent, earth and stones.

According to Plutarch, Anaximenes "called the contraction and condensation of matter cold; the laxity and rarity thereof heat."

Anaxagoras (born B.C. 499) held that the earth occupies the lowest place among the elements, fire the highest, while water and air are midway between them. Winds are produced by the rarefaction of the air by the sun's heat. Anaxagoras maintained that there was no vacuum, for although in a blown bladder there is an apparent void, yet if we press it the resistance proves that something material is within.

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* Sir G. Wilkinson's "Manners and Customs of the Ancient Egyptians."

† Ritter's "History of Philosophy."

The early physicists contented themselves with upholding their opinions by reasoning alone, and seldom resorted to experimental proof, and, so far as we are aware, this is the first recorded example of an experiment being introduced to prove an assertion.

Plato (born 429 B.C.) propounded a theory of respiration which is worthy of notice. "We breathe," he says, "after this manner. The external air compasseth us round about, and passeth in at our mouth, nostrils, and invisible pores of the body, where, being warmed, it floweth back again to the external air; by that part out of which it floweth, it again thrusteth the external air to the interior. Thus there is an unintermitted succession of inspiration and expiration."*

Zeno, the founder of the Stoics (born B.C. 362.) maintained that fire is present every where; that it is in water is proved by the fact that water is susceptible of greater cold. "It is consequently in air also, that being a vapor extracted from water, and supplied by motion of the heat which is in the water."

Chrysippus, a follower of Zeno, taught that air is divided into three regions; the highest, next to the sphere of the moon, is the hottest and most rarefied, because of the vicinity of fire; the middle region is coldest, and in it winds and clouds take their rise; the lowest region is thick, on account of the exhalations which it receives from the earth.

According to the Stoics, the elemental fire when condensed becomes air; further condensed, water; water rarefied becomes air; condensed, it produces earth. Earth is the centre of all; it is surrounded by water, this by air, and the air by fire. The Stoics believed that sound is air set in motion, and spreading its circles, like those which are produced when a stone is thrown into water.

Aristotle (born B.C 384) wrote a number of treatises on Physics, and the theories which he propounded, and the explanations which he gave of various phenomena, were almost universally admitted till the time of Galileo, and many of them till a much later period. Galileo was the first who ventured to oppose Aristotle's philosophy, and he met with much opposition, and had many obstacles to overcome before he could induce philosophers to give up any of the teachings of the master they had so long followed.

According to Aristotle † the earth is a sphere, enclosed by a sphere of water, this by one of air, and the air by fire. There is a fifth element, the ether reaching as far as the moon, but also contained within living creatures, for vital heat is not actual fire.

Each of the four elements was distinguished by two properties, and one attribute of each was common to another element; thus:—

Fire is hot and dry.
Air is hot and moist.
Water is cold and moist.
Earth is cold and dry.

Air is said to be hot and moist because Aristotle thought that it was produced from water by heat, and this was believed for many succeeding centuries.

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* Stanley's "History of Philosophy."

† The works of Aristotle, translated by Thomas Taylor.

The elements were capable of being converted the one into the other, if (said Aristotle) the dry of fire is vanquished by moist, air will be produced; if the hot of air is vanquished by cold, water will be generated from air; so fire is generated from air and earth, for "flame is especially fire, but flame is burning smoke, and smoke is from air and earth."

Aristotle did not admit Plato's theory of respiration, but conceived that air is received into the heart and acts by cooling the blood. Animals are suffocated in a confined space, because the air rapidly becomes hot, and can no longer cool the blood.

There is no vacuum. Air is a material substance, possessing weight, and every where present. That it possesses weight is proved by the fact that a bladder when inflated weighs more than it did before inflation. Simplicius, a philosopher of the sixth century, the commentator of Aristotle's works, tells us that Ptolemy, the mathematician, states that neither water nor air have any weight "in their own places," by which we suppose he means air weighed in air, and water in water. Ptolemy's experiments did not confirm those of Aristotle, for he found that a bladder weighed less after inflation than before.

Simplicius, from the most accurate experiments he was able to make, found that an inflated bladder weighed the same as before inflation, and adds that Aristotle probably filled the bladder he used by blowing into it, and thus introducing very moist air.

And this was probably the case, for a bladder weighed in air weighs the same before inflation with air as after. According to the theorem of Archimedes, a body immersed in a liquid loses a portion of its weight equal to the weight of the liquid which it displaces; hence an uninflated bladder weighed in air loses a weight equal to the weight of air which it displaces; the same bladder inflated and weighed under similar conditions loses the same weight as before, plus the weight of air which the air within it displaces; but the air it contains obviously displaces as much air as its own volume occupies, *i.e.*, a weight of air equal to its own weight; therefore the bladder must weigh the same whether inflated or empty.

The principal physical properties of the air known to the ancients, and the mechanical contrivances which work by means of rarefied or compressed air are given us by Hero, of Alexandria.

Hero, who flourished about (177 B.C.,) was the author of a treatise on pneumatics, which is believed by many to be a compilation from older works.

We must all admit, he says, * that air is matter, because if we invert the open end of a vessel having but one opening in water, we find that the water does not enter, but if a hole is now bored in the upper part of the vessel, the water rushes in and the air escapes, as may be felt, for if we place our hand over the orifice we perceive a rush of wind, which is moving air.

Hero was well aware of the elasticity of the air, for he tells us, there are void spaces between the particles of air, just as there is air between the particles of sand on the sea shore; when the air suffers compression the particles are forced into the vacant spaces, and when the pressure is removed they return again to their former position on account of their elasticity, just as horn shavings or sponge would under similar circumstances.

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* "The pneumatics of Hero, of Alexandria," translated by Bennett Woodcroft.

In speaking of the way in which cupping-glasses act, Hero tell us that the fire which is made to burn within them consumes and rarefies the air, in the same way that water is rarefied by fire and caused to become air.

A great number of the appliances which Hero mentions work directly or indirectly by means of a siphon. The siphon is an instrument of very ancient invention. Sir G. Wilkinson states that it was known in Egypt in the time of Amunoph II., (B.C. 1450.) The water of the Nile is muddy, and it is necessary to allow the sediment to deposit before drinking it. A reed was sometimes used to suck out the supernatant fluid, and it probably happened long ages ago that a man sucked out the water by a bent reed, and found to his suprise that the water continued to run after the sucking had ceased, and in this way perhaps the siphon was first discovered. The name siphon is derived from an oriental word signifying "to suck out with the breath."

According to Hero, a liquid rises in one leg of a siphon when suction is applied to the other, because when we have sucked out the air we become fuller than before, pressure is exerted on the air near us, it is communicated to the liquid in which the siphon is immersed, and compels it to rise in one leg of the instrument to fill the void space therein.

Among other inventions, we find that of a water-jet acting by compressed air. A strong globe of metal has a pipe fitted to it, reaching nearly to the bottom of the vessel, and terminating above in a jet, capable of being closed or opened at pleasure. In a small aperture in the globe a tube containing a valve opening inwards is placed; a solid plunger works in the tube, by which air is forced into the globe. It is obvious that the plunger had to be removed from the tube after each stroke, to allow air to re-enter. When a quantity of air was forced into the vessel the jet was opened, and the water rose to a considerable height. The valve consisted of two square pieces of bronze about three-fourths of an inch in the side, the surfaces of which were ground so that neither air nor water could get between them when closed. An orifice one-third of an inch in diameter was made in one of them, and they were united by a hinge.

A machine for raising water is described very similar to our modern fire-engine. Two cylinders of metal open above, and having below spherical metal valves opening inwards, are placed in water. From the cylinders proceed two pipes, which communicate with a third vessel by means of valves opening within it. To this vessel a long pipe is fitted. The cylinders are supplied with solid plungers, which when worked will obviously force water from the cistern through the long pipe.

Under the title of "a water-jet actuated by compressed air from the lungs," Hero mentions an arrangement exactly similar to an ordinary wash bottle, except as regards the tubes, which are both vertical.

A number of very ingenious contrivances, worked for the most part by compressed air, fill up a great part of Hero's pneumatics. Many of these are of no practical use, but remind one more of conjurors' tricks than of mechanical appliances. Take the following as examples:—

"A steam boiler, from which either a hot blast may be driven into the fire, a black-bird made to sing, or a Triton to blow a horn."

"An automaton, the head of which continues attached to the body after a knife has entered the neck at one side, passed completely through it, and out at the other. The animal will drink immediately after the operation."

(To be Continued.)

Monster Steam Hammer.

From the Journal of the Society of Arts, No. 593.

Messrs. R. and W. Morrison, of Newcastle-on-Tyne, have just forged what is believed to be the largest and most powerful steam hammer in the world, for the Russian Government. The piston-rod, to which the hammer is attached, weighed no less than 42 tons in its rough forged state, and now, when dressed down to the required dimensions, it has only been reduced to 35 tons. The length of the piston-rod is 38 ft., the diameter 2 ft. 4 ins., having a stroke of 14 ft. 6 ins., the piston being 6 ft. 8 ins. The forging of this mass of metal occupied 44 days. The cylinder for this hammer was cast at the Elswick Engine Works. Its diameter inside is 6 ft. 8 ins., its weight upwards of 40 tons. The two standards weigh nearly 40 tons each.

On Artificial Illumination. By EDWARD FRANKLAND, Esq., F. R. S.

Ten years ago the speaker gave in this Institution a sketch of the condition of Artificial Illumination. Since that period but little progress had been made towards perfecting those processes of producing artificial light at that time in general use. Nevertheless, the improvements effected in the production and application of the electric light, the increase of our knowledge both of solar and terrestrial light, and especially the discovery of an entirely new illuminating agent, threatening to produce a revolution in lighting almost equal to that which was the result of the invention of gas—rendered it desirable again to review this subject after the lapse of a decade.

In glancing at the improvements effected in the different departments of artificial illumination, those which relate to the electric light were first noticed. The electric light is produced by the interruption of an electric current flowing through good conductors; by this interruption the current is made to leap across a space provided in its circuit. The boundaries of this space, in the ordinary electric light, are formed by two portions of gas carbon, and during the passage of the electric current across it, the two pieces of carbon are heated to a most intense degree, and far beyond that produced by any other means at our disposal. The great scource of illuminating power was shown to be the ignited ends of these two pieces of gas carbon, and not the space between them.

Such being the nature of the electric light, the principal improvement which has been effected in it, during the past ten years, consists in the production of the electric current through the intervention of heat and mechanical force, by what is termed magneto-electricity. More than thirty years ago, Mr. Faraday produced a spark from the ordinary permanent magnet. Here, said the speaker (producing a magnet) is the very cradle of this magneto-electric light, and this is the very magnet with which Mr. Faraday operated. This improvement constitutes one of the greatest steps in advance which have been made in the application and production of the electric light. By the combustion of coal a certain amount of mechanical power is obtained, which is applied to the rotation of masses of iron in the neighborhood of very powerful permanent magnets. In this way, currents of electricity are produced, and these are thrown together, and made to circulate through a system of conductors: in fact there is no difference, as regards the effect at the carbon-prisms, between the action of this magneto-electric current, and the ordinary electric current produced by the chemical action which takes place in the voltaic battery. But this improved mode of producing the electric force demands less manual labor; the mechanical work being performed by a steam engine, which causes the rotation of the armatures.

This magneto-electric apparatus, as perfected by Mr. Holmes, has now been in use for upwards of two years in the South Foreland and Dungeness lighthouses, where, as well as in other similar beacons, the production of a light of the greatest intensity is of the greatest importance to the mariner. During this time it has performed its office remarkably well, and without a single instance of failure; thus proving itself well adapted for the purposes of lighthouse illumination.

For domestic light it has not yet been brought into use; its expense, doubtless, at the present moment, being far too great to admit of its being employed in this way: but where a light of great intensity, regardless almost of the question of expense, is required, as in the case of lighthouses, this magneto-electric light can scarcely be too much prized.

The speaker also described the mercurial electric light, but considered it inferior both in brilliancy and steadiness to that procured by the passage of the electric current between carbon terminals.

The discoveries and improvements connected with gas as an illuminating agent were then considered. It is of great importance that gas, as a light-giving material, should be improved to the utmost possible extent, so as to make it a perfect source of light. Its conveniences are so patent to every one, its use is attended with so few discomforts, and the light is obtained with such facility, and of any desired intensity within certain limits, that there is, perhaps, no source of artificial light capable of such general application. Nevertheless, this is one of the modes of illumination which, having been long previously in use, have not made great progress during the past ten years. The sulphur compounds, which at the commencement of that time were complained of as being present in purified gas, are still there in considerable, if not in undiminished quantity, although there has recently been a process devised by means of which these impurities can be, to a great extent, got rid of. It remains to be seen whether this process be applicable, on a large scale, in the gas manufactory; but, so far as can be judged from experiments made upon a small scale, it is a process which is likely to be very effective. It is the invention of the Rev. Mr. Bowditch, of Wakefield. These sulphur compounds are irremovable in the ordinary process of purification. The gas may be passed in the usual manner over hydrate of lime, or the peroxide of iron; but this operation does not, in the slightest degree, affect the sulphur compounds in question. During the combustion of the gas, however, their sulphur is converted into sulphurous acid, which diffuses itself into the apartment in which the gas is burned, and a great deal of the discomfort of which many complain in the use of gas is due to this cause. Mr. Bowditch discovered that though cold hydrate of lime will not remove these impurities, they are to a great extent got rid of by heating the hydrate of lime to a temperature varying from the boiling point of water up to 400° or 500° Fahr., a temperature of 400° being the most convenient for the development of the effects of his process. The heated hydrate of lime converts the sulphur compounds into sulphuretted hydrogen and carbonic acid, which can then be removed by the ordinary purifying material—cold hydrate of lime. This process has been found by repeated experiments to remove all but about 2 or 3 grains of sulphur per 100 cubic feet of gas, the quantity of sulphur originally contained in the gas varying from 5 or 6 grains up to 20 grains in 100 cubic feet. Heated hydrate of lime was shown to develop sulphuretted hydrogen from the gas supplied to the Institution.

Another recently ascertained fact in connexion with gas is the discovery of a new illuminating constituent recently made by M. Berthelot. This is a gaseous body, called acetylene, which is produced under very peculiar circumstances. Unlike all other hydrocarbons with which we were formerly acquainted, an intense heat is favorable to the production of this body. For instance, it is produced when coke is very intensely ignited in hydrogen gas; and Dr. Odling has recently demonstrated that two of the constituents of coal-gas, light carburetted hydrogen and carbonic oxide, which are useless for lighting purposes, may, by means of strong ignition, be made to form acetylene, and thus become luminiferous agents. It has generally been considered important to preserve a moderate degree of heat in gas-making in order to prevent the destruction of the luminiferous hydrocarbons; but the discovery of the formation of acetylene, under the circumstances named, will render it necessary to investigate how far the production of this substance can be carried on upon a large scale, and rendered valuable for increasing the illuminating power of gas. The subject is yet in embryo; but it has an important bearing upon the future progress of gas-lighting. Acetylene and olefiant gas contain, in equal volumes, the same amount of carbon, but the former contains only half as much hydrogen as the latter; consequently, the illuminating power of acetylene is probably at least double that of olefiant gas.

The compound of acetylene with copper, is a substance not altogether devoid of interest to the gas manufacturer. When it attains a certain heat, it explodes with considerable violence, and the same effect may be produced by friction. It has been demonstrated recently, that acetylide of copper can be produced by the passage of ordinary coal-gas, containing, as it does, a trace of ammonia, through copper or brass tubes; and explosions which have taken place through cleaning out such tubes, resulting in serious injury to workmen, have been doubtless due to the presence of this substance. It is, of course, obvious that an explosion of this kind, even if slight in itself, may be communicated to explosive mixtures of gas and air (in a gas-holder or gas-meter, for instance). and may thus lead to very disastrous results. An explosion of this kind occurred a few years ago in Lancashire. A large meter had been detached, and brought into the open air; a workman was unscrewing one of the brass connexions, when the meter exploded with a loud report. The explosion could not be accounted for, as all the eye-witnesses were positive that no ignited body of any kind was near the meter.

The improvements effected in the production and application of oils for illuminating purposes were next referred to.

The animal and vegetable oils, which for ages have been employed for this purpose, have received no development of importance during the last ten years. On the other hand, new sources of this class of illuminants have been discovered, which threaten to produce a great revolution in the modes of obtaining artificial light.

One of the most important of the materials manufactured from these sources is pariffin oil, the production of which from bog coal has been carried out on an immense scale with great ingenuity and success by Mr. Young, of Bathgate, near Edinburgh. [The speaker here exhibited several lamps fed with this oil.] This new source of light is however already threatened by a formidable rival—native coal oil—which has been distilled by nature herself, and consequently does not require those preliminary processes which oil produced from bituminous coal has to undergo. These discoveries have been made principally in the United States of America, and more especially in Canada. In the latter country alone no less than twenty millions of gallons of this oil have been obtained from wells, several of which are spouting-wells. From these the oil rises, probably from the pressure of gas, to a considerable height above the surface of the ground, so as not to require pumping. The twenty millions of gallons, which represent the annual production of the Canadian wells, may. upon a moderate calculation, be said to furnish, in refined oil alone. illuminating material equal to one hundred and eighty millions of pounds of sperm candles.

The importance of such a vast amount of illuminating material so cheaply obtained can scarcely be overrated in connexion with the question of the production of artificial light. Up to the present time, the refined oil from this crude petroleum (specimens of which were exhibited) has been prevented from coming into effective competition with the original paraffin oil, owing to the carelessness with which the former has been manufactured. There is a considerable portion of light naphtha left in this oil, which renders it capable of forming explosive mixtures in the lamps wherein it is burned. Both these American oils require to be still further freed from volatile naphtha. They would then form valuable illuminating materials.

The speaker then directed attention to the following tables. The first contains the results of his experiments on the illuminating effect of these oils in connexion with that of some other materials used for giving artificial light:—

Illuminating Equivalents, or the Quantities of different Illuminating Materials necessary to Produce the same amount of Light.

Young's Paraffin oil,	1	gallon.
American rock oil, No. 1,	1·26	"
do. No. 2,	1·30	"
Paraffin candles,	18·6	lbs.
Sperm do.	22·9	"
Wax do.	26·4	"
Stearic do.	27·6	"
Composite do.	29·5	"
Tallow,	36	"

From this table was made the following calculation of the comparative cost, from different sources, of the light of twenty spermaceti candles, each burning for ten hours, at the rate of 120 grains per hour:—

	s.	*d.*
Wax,	7	2½
Spermaceti,	6	8
Tallow,	2	8
Sperm oil,	1	10
Coal gas,	0	4¼
Cannel gas,	0	3
Paraffin,	3	10
Paraffin oil,	0	5
Rock oil,	0	6½

Thus, from an economical point of view, the rock oil and the paraffin oil approach gas much more closely than any other illuminating agent hitherto invented; while the enormous quantities in which these oils are now being produced cannot fail to make them still lower in price. They may consequently be regarded as very formidable rivals of gas-light.

The following table contains the amount of carbonic acid and heat generated per hour by various illuminating agents, each giving the light of twenty sperm candles:—

	Carbonic acid.	Heat.
Tallow,	10·1 cubic feet	100
Spermaceti, Wax,	8·3 "	82
Paraffin,	6·7 "	66
Coal gas,	5·0 "	47
Cannel gas,	4·0 "	32
Paraffin oil, Rock oil,	3·0 "	29

This table shows to what extent the atmosphere of rooms is deteriorated by these illuminating agents. It shows also that, from this point of view, paraffin and rock oils stand out as the best sources of light.

The chemical and physical laws upon which all modes of artificial illumination depend were then referred to. Every method of artificial illumination depends upon the heating of certain bodies to such a temperature that they become incandescent. With gases this temperature is much higher than with liquids or solids. There is only one instance in which the incandescence of vapor is used, and that is the mercurial electric light, already alluded to. In all other cases the incandescence of solid bodies is employed. In the ordinary method of obtaining the electric light, the incandescence of solid prisms of carbon is the source of luminosity. In gas and oil flames, it is the incandescence of little particles of carbon, and in all these cases the light is produced from solid matter. The luminosity of any flame depends, first, upon the number of solid particles which exist in it at any given moment; and, secondly, upon the temperature of the flame. The number of solid particles is dependent upon the nature of the flame itself, whether it be a flame produced by the burning of bodies rich in hydrocarbons, or by the burning of bodies which are poorer in this respect. Such a flame is always affected by the pressure of the atmosphere. The higher the pressure of the air, the greater the number of luminous particles of carbon present at one time in the flame. If, after the barometer has been standing at 30 inches, it falls to 29 inches, the light of all flames is reduced to the extent of about 5 per cent. The temperature, upon which the luminosity of a flame also depends, may be increased by heating both the gas and the air supplied to it for combustion, before they are brought together to be burned.

The speaker exhibited a gas lamp which he had constructed to effect this object. It consisted of an ordinary Argand burner, with glass chimney, but furnished with an outer glass cylinder resting upon a solid plate of glass, through the centre of which the tube supplying the gas rose. Thus all the air supplied to the flame was compelled to pass down between the chimney and outer cylinder, becoming thereby strongly heated before it reached the flame, whilst considerable heat was also imparted to the gas before the latter issued from the burner. In this manner a great increase of light, with the same consumption of gas, was obtained.

The following table was exhibited, showing the effect of this hot air gas-burner in reducing the consumption of gas for a given amount of light, and thereby also the impurities and heat which are thrown into the atmosphere in which such a lamp is burned:—

	Rate of consumption per hour.	Illuminating power.
Argand burner supplied with cold air, .	3·3 cubic feet,	13 Sperm candles.
	3·7 "	15·5 "
	4·2 "	17 "
Same burner supplied with hot air, .	2·2 "	13 "
	2·6 "	15·5 "
	2·7 "	16·7 "
	3·0 "	19·7 "
	3·3 "	21·7 "

For an equal amount of light, the saving of gas = 33 per cent. For an equal consumption of gas, the gain in light = 62 per cent.

The temperature necessary to render substances incandescent may be imparted to them in various ways. It may be given directly by mechanical power, as in the "steel mill," formerly used in coal mines. Usually, however, chemical action is employed, as in gas, candle, and oil flames; or electricity is used, as in the various forms of electric light.

The conditions necessary for a good and satisfactory artificial light were now examined. In the first place, the light should contain all colors; that is, it should be capable of showing every variety of tint which will be exposed to it. This is the case with the carbon electric light, and that candle, oil, and gas flames, since the light from these sources contains all the colors of the spectrum. But there are many colors which the mercurial electric light is incapable of showing, since they are absent from its spectrum. It was also shown that all pure colors, except yellow, were perfectly black, when seen by the light of incandescent sodium vapor.

Solar light, although in so many respects superior to artificial light, is defective as regards the showing of colors. There are certain colors which cannot be seen by solar light—for instance, all the color which can be seen by the sodium flame is quite invisible in daylight. If a pigment could be made of such a yellow color as to be of exactly the shade of that produced by the sodium flame it would be absolutely black in solar light. But our pigments are all mixed colors, and no such pigment which thus entirely disappears in the light of the sun is known. But in addition to this tint of the sodium flame, there are hundreds of other tints which are also not present in the solar spectrum, and which are consequently invisible in daylight.

Although solar light is inferior to artificial light in the completeness of its colors, yet, in another respect—in the comparatively small amount of heat which accompanies its rays in proportion to the light itself—it is greatly superior to every sort of artificial light. The great amount of heat in our artificial lights is absolutely useless. It is nearly all intercepted by the humors of the eye before it reaches the retina, and, no doubt, produces that irksomeness which is felt after working in artificial light for any considerable time, and which is not experienced from daylight.

The speaker concluded as follows:—The history of artificial illumination cannot fail to impress upon us the difficulties in the way of the application of scientific discovery to the utilities of life. How long was it after the discovery of the production of gas from coal, before a manufacturer could be found to bring it into actual operation? Thirty years ago working in his laboratory at Blansko, Reichenbach showed us the process by which we could obtain paraffin and paraffin oil from bituminous coal; but the discovery remained unheeded for twenty years. More than thirty years ago, Mr. Faraday pointed out a source of the electric light in the permanent magnet; but we are only now beginning to use it for illuminating purposes. The brilliant little spark was long looked upon as a mere scientific curiosity, and is only now beginning to flash across the sea, guiding the mariner safely into harbor, or warning him from approaching a dangerous coast. How long will thermo-electricity have to wait before it receives a similar application? In thermo-electricity we have a direct transformation of the force of heat which we obtain with such great economy from coal, into an electric current, and this, by further education and development, might be rendered available in the production of the electric light. Hitherto, its application in this direction has been altogether unheeded, and yet of all sources of the power necessary for the electric light, thermo-electricity evokes this power most directly from coal. In the magneto-electric light we have the great disadvantage, that the heat of burning coal must be first transformed into mechanical power, which is made to rotate the armatures of magnets, and thus produce the necessary electric current. In this transformation of heat into mechanical power there is no less than $\frac{9}{10}$ths of the original force in the coal absolutely lost. Hence the advantage which would result from the direct application of heat to the production of the electric current.

The man of science rejoices in the discovery of truth for its own sake. He gives his results freely to the world. It is no part of his duty, it is not his function, to apply those truths to the utilities of life. Success in this direction demands quite different powers of mind. Those who possess these powers, ought also to acquire the necessary knowledge which would enable them, with more facility, to seize upon the discoveries of science, and apply them to the wants of every day life. This scientific knowledge is the link which, up to the present time, has so sadly failed in the application of science to the manufacturing arts.

Proc. Royal Inst., Feb. 13, 1864.

New Maximum Thermometer.

From the Journal of the Society of Arts, No. 583.

The novelty in this thermometer consists in the addition, near the bulb, of a small tube at right angles to the tube of the thermometer, and in connexion with the bore; and when the thermometer is placed horizontally, this tube stands perpendicularly above it. On an increase of temperature taking place, the mercury will rise in the small bore, as in an ordinary thermometer; but on the temperature decreasing, instead of receding, in the small bore, as in an ordinary thermometer, the mercury in the bulb, on contracting, draws with it the mercury from the small side tube, and then the column of mercury in the small bore registers the maximum temperature. To reset the thermometer for future observation, lower the end near the bulb, and allow the mercury to fall until it fills the small tube at the side, then the thermometer shows the temperature at the time, and is set for future observation.

Description of the Harrison Cast Iron Boiler.

From the London Engineer, May, 1864.

The boiler about to be described has been constructed with reference to the foregoing considerations. It was the object of the inventor, Mr. Joseph Harrison, of Philadelphia, U.S., to provide great strength against bursting, and to obtain, also, a large extent of heating surface in proportion to the weight and external dimensions of the boiler. It was important, moreover, to obtain perfect circulation for the water. An experience of several years in America, and for upwards of two years in London and Manchester—in one case with a boiler supplying steam to the extent of 200 indicated horse-power—has proved that these objects, as well as other important advantages, have been secured. It will not be necessary to describe now the forms which the parts of the boiler received in the earlier experiments, several years ago, but these led to the adoption of hollow cast iron spheres, connected by hollow necks, and secured together by bolts, as shown in longitudinal elevation in Plate II. Each of the castings includes four spheres, each 8 ins. in external diameter, $\frac{3}{8}$in. thick, and connected by necks of 3 ins. opening. Each of these castings is called a "unit." Each "unit" of four spheres has eight openings, 3 ins. in internal diamater, the edges of these openings being faced up to a true surface, so as to bear fairly upon the corresponding faced surfaces of the adjoining "units." Each joint has a shoulder and socket, so as to "steady" the units in their place, and steam tight caps are provided to cover the external openings, while the whole series of units, forming a slab of rectangular or other form are held together by bolts of $1\frac{1}{4}$ ins. diameter; these bolts passing inside the "units" and through the water or steam which they contain. Although the arrangement is simple, it will be better understood from the drawings, and from the units exhibited to the meeting, than from a purely literal description. Each slab, of whatever number of units it may be formed may be regarded as a separate vessel, throughout which the water or steam can circulate freely, both vertically and longitudinally. Any number of slabs may be placed side by side, in the same fire place, and they are connected together by a feed water pipe at the bottom and by a steam pipe at the top. There are eight slabs in the width of the boiler shown in end elevation. The water level is usually maintained so that about two-thirds of the whole number of spheres will be constantly filled with water, the remaining spheres forming a steam space. The full force of the heat is not allowed to come upon those of the spheres which contain only steam, but small fire-brick screens are so placed between the slabs a little below the water level, as to confine the direct action of the heat chiefly to the spheres filled with water. The upper spheres are at the same time enveloped in an atmosphere so hot as to insure the complete drying of the steam. The slabs are fixed with an amount of inclination, in the direction of their length, sufficient to insure the complete drainage of all the spheres when the boiler is blown out. This inclination serves, at the same time, to bring the largest body of water to where the action of the heat is most direct, and to provide the largest steam space over that part of the boiler where ebullition is probably least active. The earlier experiments have shown that, although the "units" may be bolted together into slabs of a total length of even 20ft., a length of 9ft. is preferable, as the strain upon the bolts is correspondingly less, and as, in the latter case, there is no observable tendency to sag, the complete tightness of the joints is thereby insured.

The spheres weigh each, about 22½ lbs., a "unit" of four spheres weighing rather more than 3 cwt. Thus there are as nearly as may be, one hundred spheres to the ton, and it has been the habit, thus far to rate the boilers by their weight, as a 4-ton boiler, an 18-ton boiler, &c. The nominal horse-power of the cast iron boiler may be generally taken as three times its weight in tons. Thus a 10-ton boiler may be rated as of 30 nominal horse-power, and from experiments it appears that a boiler of this weight may be counted upon to evaporate 40 cubic feet of water per hour, corresponding to about 80 indicated horse-power. Each sphere contains seven pints of water, a "unit" of four spheres containing 3½ gallons. The external surface of each sphere is rather more than 1¼ square feet, and the internal surface a little more than 1⅛ square feet. In round numbers it may be said that each sphere presents a square foot of heating surface, and contains a gallon of water; while a ton of one hundred spheres represents three nominal horse-power, the proportion of weight to power being about the same as in Lancashire boilers of the ordinary type.

It cannot be said that cast iron is, itself, a strong material for boilers, yet it will appear that, in the form already described, it affords greater absolute strength against bursting than is possessed by any form of plate-iron boiler now in use. The "units" are cast upon green sand cores so placed that they cannot alter their position in the flasks by any force short of what would be sufficient to crush them to pieces. The thickness of metal in the spheres is, therefore, every where uniform, as has been proved by breaking great numbers of "units" taken at random. In a unit of four spheres, each sphere having an internal diameter of 7¼ ins., the whole area of the plane in which a bursting pressure would act, is 220 square inches, while the least section of iron resisting this pressure, in the same plane, is 27½ square inches. The iron employed is an equal mixture of Glengarnock, Carnbroe, and scrap, a mixture selected for its free running quality, and which is much used for small machinery castings. Its tensile strength may be safely taken at 5½ tons per square inch. At this rate the bursting strength of the units would be 1540 lbs., or nearly 14 cwt. per square inch. The first experiments actually made to test the bursting strength of the units was made upwards of two years ago, in Brussels, at the request of the Belgian Minister of Public Works. In this case a pressure of 98 atmospheres, or 1440 lbs. per square inch was applied. This was as high as the force pump employed could go, but the spheres were not burst. When recently in Manchester, the author desired that a further series of experiments might be made with the same object. The arrangements for this purpose could not be completed before he was compelled to return to London, but Mr. Beyer kindly allowed the experiments to be made at the Gorton Foundry, and he deputed Mr. R. H. Burnett, then engaged in that establishment, but who is now Locomotive Superintendent of the Metropolitan Railway, to conduct them. The first of these experiments need not be recorded, as it was made with a hydraulic press gauge registering up to 4 tons per square inch, but which was subsequently found to be most incorrect. A high pressure gauge of Schaffer and Budenberg's, graduated to 1000 lbs. was then attached, to one of the units to which the caps had been accurately ground, and a water pressure was then applied by means of a force pump. The pointer of the gauge passed the 1000 lbs. mark to an extent indicating from 1150 lbs. to 1200 lbs., but the spheres did not burst. The Schaffer gauge was then compared with a Bourdon gauge marked to 500 lbs., and up to this point the gauges agreed within 10 lbs. per square inch. From a calculation of the weight applied to the force pump lever, and from the dimensions of the pump, Mr. Burnett estimated that the total force applied was about 1470 lbs. per square inch. Another casting of four spheres was afterwards tested in the same way, the pointer of the Schaffer gauge passing far beyond the 1000 lbs. mark. In this instance three men, instead of two, as in the first trial, were put to work the pump, but the spheres did not burst. The castings were subsequently broken with a sledge, and showed a uniform thickness at all parts, and a good quality of iron.

A safety valve was then arranged with the intention of ascertaining the bursting strength of the spheres. The valve was, square ½ in. on a side, and, therefore, of ¼th square inch area. Its head was 1¼ ins. in diameter, and was ground carefully to its seat. The spheres were burst at a pressure calculated at 1850 lbs. per square inch, but on comparing the safety valve with a pressure gauge it appeared that water must have worked its way over the ground seating of the valve, and that the true pressure could have hardly been so much. The seat of the valve was then turned to a diameter of ⅝in., and the spheres were burst, at a calculated pressure of 1650 lbs. per square inch. Even here it was found that some water must have worked under the valve seating, thus exerting its pressure upon an area greater than one-fourth of one square inch. The experiments with the safety valve were not, therefore, altogether satisfactory, but there appeared no reason to doubt that the bursting pressure was not far short of 1500 lbs. per square inch. All these experiments were made upon castings having their covering caps ground carefully to them, and the bolts were only about 9 ins. long between the caps covering the opposite openings of the units. When, however, a slab of perhaps one hundred spheres is bolted together, the bolts being upwards of 9ft. in length, the application of a strain considerably below the bursting pressure so stretches the bolts as to cause the joints to open every where and relieve the pressure. In this way every joint becomes a safety valve. This never occurs with any practicable steam pressure, but it did take place in many of the earlier experiments made to burst the spheres, although leakage seldom commenced until a strain of nearly or quite half a ton per square inch had been applied.

All these experiments were made with new castings, and at the time they were made no other spheres could be had which had been more than twelve months in use; and the condition of these was clearly the same as when new. It would appear, therefore, that the boiler now described possesses the same factor of safety under a pressure of 225 lbs. per square inch as a 7ft. Lancashire boiler under a pressure of 50 lbs. If, however, one of the units of the cast iron boiler should burst it could not do more than empty itself, and open one or more 3 in. apertures into the units adjacent to it. If, however, an ordinary boiler cantaining, say, 20 tons of highly heated water in one compartment, should burst, the consequences would be most disastrous. In some of the earlier boilers of the kind now described the setting was such that an excessive strain was brought upon one or more joints, and here, in order to prevent leakage, the bolts had to be tightened with great force. In two or three cases castings forming a part of the boiler were thus cracked from one joint to another. The consequence was an escape of steam or water, but no further damage ensued. In one of these instances a unit, thus cracked, was worked continuously for three days; and it might perhaps have worked for a still longer time, but it was thought prudent, for obvious reasons, to replace it by a sound casting. No instance of a fracture has occurred in the cast iron boilers with the present mode of setting, and all the boilers of this kind yet erected are quite free from leaks at the joints.

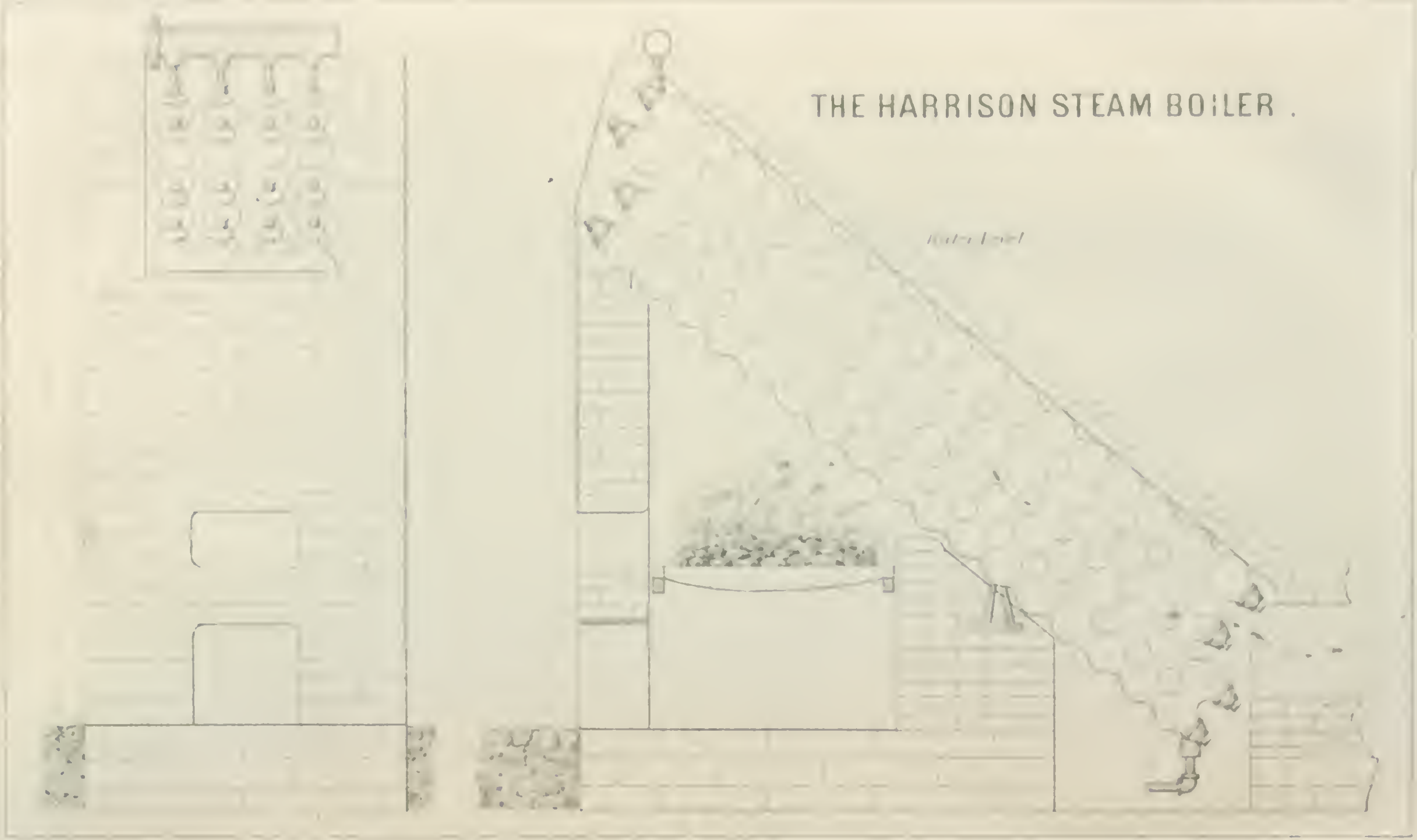
It will be remembered that the bolts have a strength much beyond even that at which the units would burst. The bolts, however, are under a certain initial strain before any pressure is raised in the spheres. This amount of initial strain is known and under control, as, in screwing up the slabs, a 27-in. wrench is employed, and the strength of but one man is applied to it. If, however, a great strain be put upon the bolts, the crushing strength of the castings is found to be greater than the tensile strength of the bolt. In a series of experiments, made by Mr. Luders, a slab of units, bolted together to a length of 9ft. was screwed up with great force. A wrench, 10ft. in length, was employed, and the force of three men applied. In every case the castings were compressed to the extent of ⅛in., when the bolt commenced to stretch, and after elongating 1⅛ins. it broke. This experiment was repeated twelve times with the same result, the castings remaining uninjured. The castings, when laid loosely upon a brick pavement, require a powerful overhead blow, from a heavy sledge hammer, to break them. They may be heated in a forge to a bright cherry red color, and be then plunged in cold water without cracking. Some of these castings, now at the Museum of Patents, South Kensington, were thus heated and quenched seven times in succession, and they still give out a clear, ringing sound when sharply struck with a hammer. There is no doubt that the iron was most seriously injured by this treatment, yet it might have been expected that the spheres would have broken to pieces. In one case while heated to a bright red, the casting was half filled with cold water. The endurance of the spheres under this treatment is to be referred to their form, and to the tough quality of the metal from which they are cast. A sharp blow with the edge of a hammer indents the iron nearly the same as if it were boiler plate.

It might have been apprehended that the expansion of the castings, when in service in a boiler, would be such as to cause unequal strain upon the joints. No leakage at any of the joints of a single slab of castings can be detected, however; and as each slab is supported chiefly at its lower corner, and as all the slabs of a boiler are separate from each other, except at a single point above and below, where the steam and water connexions are respectively made, it is found that the slabs are under no injurious strain. All the spheres have, too, a considerable amount of elasticity under strain, which would assist in compensating for unequal expansion did it exist. These conclusions, as to the effect of expansion, are derived from an experience of two years and a half with a Harrison boiler, of 12-horse power, at the Chemical Works of Messrs. Denton, at Bow Common, London; two boilers—one of 50-horse and one of 12-horse power—at the Vulcan Works of Messrs. Hetherington and Sons, Manchester; and from a 12-horse boiler at the Harrison Boiler Works, Openshaw, Manchester. Messrs. Hetherington's boilers are often worked collectively up to 200 indicated horse-power and the first one was erected at their works about eighteen months ago. Boilers of this construction were originally bolted up in slabs 25ft. long, where considerable power was required; but in such cases it is now preferred to employ two or three slabs, one behind the other, as shown in Fig. 1, Plate II, each slab being about 9 ft. long. When this arrangement was first employed, the steam pipes, connecting the front and back slabs, were made of cast iron, and of a form which did not allow of the unrestricted expansion of the slabs. Some of these pipes cracked; but they are now made of wrought iron, and of a form to yield readily to a moderate strain.

Mr. Harrison had, from the first, counted upon entire freedom from corrosion of the spheres, and the experience thus far has borne out this anticipation. Cast iron, as is well known, endures much better than wrought iron under the action of flame, water, and other corrosive influences. It need hardly be said that plate iron would, if employed for gas retorts, be immediately burnt through, yet until the introduction of clay retorts, cast iron answered very well. The pipes for heating the blast of blast furnaces were originally made by Mr. Neilson of plate iron, but, although the blast was then heated to but 350 degs. it became immediately necessary to resort to cast iron heating pipes. The superior durability of cast iron forge tuyeres, especially where made hallow and lined with water, is well known. Mr. Jaffrey, the engineer to Messrs. Thomas Richardson and Sons, of Hartlepool, has employed cast iron superheaters with much success, and he has informed the author that four superheaters upon his plans, fitted on board the steam colliers Berwick, Killingworth, Wearmouth, and Earl of Elgin, showed no sign of corrosion, when examined in October last after four years of almost constant service. Mr. Jaffrey's assistant reported to him that the superheaters "looked almost as well as on the day they were put in; there was not the slightest sign of external corrosion, and the pipes when cleaned look like new castings. The cast iron connexions of the Earl of Elgin are just the same; the heat does not seem to have taken any effect on them." Mr. Jaffrey adds, "I am quite satisfied in my own mind as to the value of cast iron where a strong heat is applied, as these and kindred instances establish." In the case of the Harrison boiler many castings have been purposely removed and examined, but their weight is the same as when they went in, and the joints show no degradation of their original surface.

The point, in connexion with the boiler now described, which caused most apprehension in the first instance, was that of maintaining a clean surface within the spheres. The cast iron boiler may be said to belong to the family of water tube boilers or those having small water cells. The water tube boiler is at least sixty years old, for Arthur Woolf fitted one in Meux's brewery, in London, in the year 1804. In the same year John C. Stevens, of New York, worked a small screw steamboat on the river Hudson, the engine of which vessel was made by Boulton and Watt, while the boiler had eighty-one water tubes 1in. in diameter and 2ft. long. From the first, however, water tube boilers have generally failed on account of defective circulation, and from the difficulty of keeping the tubes free from internal deposit. Many attempts have been made to remove this difficulty. Mr. William Henry James long ago proposed to employ circulating pumps, in addition to the ordinary feed pump, to maintain a constant circulation of water through the tubes. The boilers of the first American steam fire engines were thus constructed, and Mr. Spencer, and Mr. Russell some time since, gave descriptions of such a boiler to this Institution. Other forms of water tube boilers have been made with different means for promoting a circulation of the water, but in all cases the whole of the inorganic matter contained in the feed water must remain in the boiler, unless it be blown out while working; and in case of some salts held in solution by ordinary boiler waters, these are inevitably and almost irremovably deposited upon some part of the internal surfaces. The Harrison boiler forms no exception to the general experience in this respect. The water with which Messrs. Hetheringtons', and indeed most boilers in Manchester, are fed, is such as to form a hard scale, ⅛in. thick, after a few weeks time. A tool had been contrived with steel scrapers, so hinged, that it might be entered through any of the openings in the cast iron boiler, and be then forced out to the internal circumference of the spheres. By then working this tool within the spheres the scale would be removed, so that it could afterwards be blown out. Unexpectedly, however, no occasion has arisen for the use of this tool. It was found that the supply of steam continued good without it, and that none of the spheres were overheated or leaking. The boiler was regularly blown out at the end of every week. After ten months' work it was desired to increase the boiler power of Messrs. Hetheringtons', and as the large cast iron boiler then in use there was formed of units having only two spheres each, it was thought best to replace it with a new boiler having four spheres in each unit, with the exception of those employed for breaking joint, which had two spheres as before. On taking down the old boiler little or no scale was found in any of the spheres, some of which, in the same condition as when taken down, are now exhibited to the meeting. One taken from Messrs. Hetheringtons' twelve horse boiler was broken by the author, and having been marked by him is also exhibited. It was purposely taken from near the bottom of the boiler, and it had worked constantly for eight months without any examination. It will be seen that the broken pieces, which are in the same condition as when the author took out and broke the casting, contain no scale. In taking down the old boiler, however, a lump rolled out of one of the spheres, and this the only one of the kind found, is also exhibited, although it has been accidentally broken in two. It consists of scales, seldom larger or thicker than a sixpence, loosely cohering together by a clayey deposit from the water. In the mass it is very friable, but the fragments of scale are, of themselves, of the same obdurate sulphate of lime as that which hardens in nearly all other boilers in the same district. Yet they have in every case separated from the iron before attaining a thickness greater than one-sixteenth of an inch. It cannot be because the boiler is of cast iron that it so readily sheds its scale; Trevithick's boilers, and the cast iron boilers long made by Mr. Hall, of Dartford, and known as "elephant boilers" enjoyed no immunity in this respect, when compared with wrought iron boilers fed with the same kind of water. In the Lancashire district, too, the cast iron pipes of Messrs. Green's "Patent Economizer," are known to be subject to choking with scale in the same manner as if they were made of wrought iron. In some cases they become completely choked, and can only be cleared by a boring tool. The fact that the spheres of the Harrison boiler shed their scale is not referable, therefore, to the material of which they are made. It might, perhaps, be argued that the water is occasionally driven from the internal surfaces, and that the consequent expansion of the spheres, and their subsequent contraction on the return of the water, would account for the loosening and breaking of the scale. But the spheres show no evidence, as in this case they might be expected to do, of the irregular action of the fire, and, too, those of the spheres which are placed far behind, where the action of the heat is moderated, are equally free from scale. It appears to be

THE HARRISON STEAM BOILER.



more probable that, as the spheres expand at all parts, and, in cooling, contract equally at the same parts, the scale is detached and crushed in this process of contraction. If this conjecture be correct, the unexpected separation of the scale may be attributed to the form and dimensions of the spheres themselves. Whatever explanation may be offered it is certain that, with foul water, and with such as gives much trouble with other boilers, the scale breaks freely off, and into small pieces, in the cast iron boilers now described. This is, perhaps, one of its most valuable properties; but it was quite unforseen. It would not be prudent to anticipate the result in the case of the application of the Harrison boiler to marine purposes; but with all the land boilers upon the plan now described, it has been found that, with a blowing off once a-week, they may be worked indefinitely without any formation of scale. The description already given will show how readily the cast iron boiler may be laid open for examination, and it was recently thought expedient to open the large boiler at Messrs. Hetheringtons' for this purpose. A small quantity of loose and broken scale—perhaps a tablespoon full—was found in each of the spheres examined, but their internal surfaces, so far as they could be seen, were entirely clear.

The evaporative efficiency of the cast iron boiler depends, as in the case of all other boilers, upon the amount of heating surface exposed in proportion to the consumption of a given weight of fuel in a given time. The boiler, by which Messrs. Hetheringtons' works are now driven, supplies an amount of steam which a single Lancashire boiler 7ft. in dameter, 30ft. long, and weighing 14 tons, was found inadequate to produce. Both the original and the present boiler are in connexion with a chimney 165ft. high, and which affords an excellent draft. The Lancashire boiler had two flues 2½ft. each in diameter, and enlarged at the fire-place to 3ft. The area of the fire bars was 36 square feet, and the total run of the heat was 90ft. in length. The cast iron boiler now in use has about 1800 spheres, weighing 18 tons, and presenting about 1600 square feet of surface in the water spheres, and about 700 square feet in the steam spheres. The area of fire-grate is 33 square feet. The usual quantity of water carried is 147 cubic feet, or rather more than 4 tons, the quantity usually carried in the former Lancashire boiler being nearly 20 tons. The external dimensions of the present boiler are considerably less than those of the Lancashire boiler formerly employed. Rather more than 3 cwt. of coal are now required in raising 50 lbs. of steam from cold water, and the time occupied is about half an hour. In order to ascertain the exact evaporative efficiency of the boiler, it would be necessary to begin the observations when it was in full work, and to continue them uninterruptedly for a considerable time. As the boilers are now worked, the fires are lighted on Monday morning and let down on Saturday afternoon, but they are banked every day at breakfast time, at noon, and at night. The mass of brick-work, which is thus alternately heated and cooled with the boiler, is very great, and the heat which enters it, and which on stopping, is for the most part wasted, is correspondingly large. Except at the beginning of the week, the temperature of the water in the boiler on starting in the morning is at least 212 degs., while the feed water from the hot-well is usually between 90 degs. and 100 degs. On the 13th, 15th, and 16th of February last, the author made a series of careful observations upon the working of Messrs. Hetheringtons' boiler, more especially to ascertain its evaporative efficiency. The coal, of good quality, and from the Oldham pits, was carefully weighed and the feed water was made to pass through one of Worthington's water meters on its way to the boiler. The indications of the meter were ascertained to be accurate, one of the best tests being its registration of 147¾ cubic feet, in filling the boiler to a point known to exactly correspond with that quantity. Between 5h. 40min. in the morning, and 12h. 55min. p.m., with an interval for breakfast, the whole consumption of coal was 38 cwt., and of water 442·7 cubic feet. If the quantity of water evaporated were to be divided by the gross consumption of coal the quotient would be 6½ lbs. of water only evaporated per pound of coal; but in heating the boiler and its contents to the working point, and in the loss at breakfast time, the consumption of coal was such that 24 cwt. were burnt while evaporating the first 200 cubic feet of water, while the remaining 14 cwt. held out for the time during which 242·7 cubic feet of water were evaporated. Allowing for the lowering of the fires at the hour for stopping work, the quantity of coal actually expended in evaporating the 242·7 cubic feet of water may be taken at 16 cwt., corresponding to an evaporation of 8·43 lbs. of water per pound of coal. Towards the end of the trial, in a period of 1h. 40min., 8 cwt. of coal were burnt and 142·7 cubic feet of water were evaporated. But as this would correspond to an evaporation of 9·91 lbs. of water per pound of coal, it is probable that a portion of the water was vaporized at the expense of the heat already in the water, and in the boiler and its brick-work setting. That this was the case is proved by the fact that in the last 50 minutes of the trial only 2 cwt. of coal were burnt, while 80 cubic feet of water were evaporated, a proportion which were the total evaporation due to the quantity of coal actually burnt within the same period of time would have corresponded to the evaporation of nearly 22½ lbs. of water per pound of coal, which, it is needless to say, would have been impossible. In making these observations there was always the uncertainty attending the quantity of coal assignable to heating the boiler and its contents and setting, up to the working temperature, and that due to evaporation alone.

On the 15th of February, starting with water at about 45 degs. temperature, 5 cwt. of coal were consumed in raising steam to the working pressure, the time occupied being half an hour. Including the coal burnt in raising steam, 2 tons 18 cwt. were consumed during the day, and 657¼ cubic feet of water were evaporated, corresponding to 6·3 lbs. of water per pound of coal. But as a greater quantity of heat was manifestly left in the boiler, and brick-work on stopping than when work was commenced in the morning, and as there was a waste of heat during the breakfast and dinner hours, not more than 2½ tons of coal, at most, can be fairly charged to the water evaporated. It was the opinion of those who witnessed these experiments that even this was too large an allowance of coal. It corresponds to 7·3 lbs. of water per pound of coal. The evaporation, from noon until the hour of stopping at night, averaged 7·56 lbs. of water per pound of coal; while, for the last four hours of the experiment, the apparent rate of evaporation was 8·71 lbs. On February 16th, steam having been kept in the boiler over night, the consumption of coal from 6 a.m. till 2½ p.m. was 2 tons 6 cwt., which included the loss during the breakfast and dinner hours, and the evaporation was 519 cubic feet, corresponding to 6·3 lbs. of water per pound of coal.

In a whole week, of 57½ hours, an average of 77 cubic feet was evaporated per hour, the maximum evaporation being about 82 cubic feet per hour. The average consumption of coal was 6 cwt. 1 qr. per hour, corresponding to 6·85 lbs. of water per pound of coal, but allowing for the sources of loss already pointed out, nearly 8 lbs. may be taken as the effective rate of evaporation. The temperature of the escaping gases, as indicated by Gauntlett's pyrometer, was about 600 degs. on the average, the steam of 50 lbs. pressure, showing its normal temperature of about 300 degs., by a thermometer inserted for the purpose. The average rate of combustion was 21 lbs. of coal per square foot of fire-bar per hour. When the fires were not driven so hard the rate of evaporation per pound of coal was increased, and the temperature of the escaping products fell to 525 degs. The flame penetrated freely between the spheres for a distance of eight or ten feet from the bridge, and three-fourths of the whole evaporation effected probably took place within this distance. The spheres in the slabs at the back of the boiler were generally covered with a light coating of soot, which was swept off every week, all the spheres being within easy reach for this purpose. Soot never formed, however, upon the spheres near the fire. The water level was very steadily maintained within a small range of oscillation. As feed water entered the boiler at the back, there could be no doubt, when it stood at its proper height in the glass gauge in front, that its level was properly maintained throughout the whole length of the boiler. A small cock, tapped into one of the steam spheres a short distance above the water level, showed damp steam, indicating a vigorous circulation of the water below, but, in the engine room, the steam blown from the cylinder cocks was quite dry, showing the value of the superheating surface formed by the upper or steam spheres of the boiler. In a recent instance an inclination of 45 degs. has been adopted for setting the slabs, and it is believed that this permits of a still better circulation of the water, or, rather, an easier disengagement of the steam from the water.

A lengthy paper might be prepared upon the mode of making the castings of the boiler and of forming the joints. The system pursued in the foundry is such that, with green sand cores, the units are moulded with about the rapidity and economy of plate moulding. The cores are well hammered on the eight prints upon which they rest in the sand, and there is no chance of their being displaced in pouring or otherwise, except by a force sufficient to break them in pieces. The two halves of the moulding boxes are drawn apart in such a manner as to prevent any chance of breaking down the sand, and a little or no sleeking is required. Each casting is critically inspected, but, as the moulders are not paid for imperfect castings, very few are made. The joints are faced up by special and powerful machinery. The spheres are 9 ins. from centre to centre, and these machines not only preserve that distance exactly between the openings on the same side of each unit, but they face the joints so that those on opposite sides of the unit shall be precisely 9 ins. apart. Each machine has two headstocks, with eight spindles and rose-cutters in each headstock, and the castings being immovable and held in a clamp, are faced in pairs, a roughing cut being taken on the eight joints of one casting while a finishing cut is going on upon the eight joints of the other. Twenty-five tons of castings pass under the roughing cutters before they require grinding, and 100 tons of units are completely faced before the finishing cutters require to be re-ground. By a special adjustment, the finishing cutters may be set up in their spindles by the one-thousandth part of an inch, when, from the dulling of the cutting edges, the distance between the opposite joints of the units are found to exceed the length of the standard gauge of that quantity. Of an infinite number of castings any two will exactly correspond. When placed together the bearing surface at each joint is $\frac{3}{16}$in. wide all round, and it is finished with a truth not inferior to that of a good slide valve face. If a sheet of oiled paper be placed upon a smooth board, and two units be placed upon it, one above the other, they may be filled with water, without any perceptible leakage at the joints even after the lapse of a week.

In conclusion it is believed that the boiler now described possesses several important advantages. It is believed to be absolutely secure from explosion, and, so far as experience has gone, free from any liability to choke with scale. It is durable, easily taken apart and put together, and it may be erected in almost any form, adapted to that of the space in which it may be necessary to place it. The parts are very portable, and they may be taken through any opening where a boy can pass. Any part of a boiler upon this construction may be readily renewed if necessary, and an existing boiler may be at any time readily enlarged, and to an indefinite extent, by adding to the number of slabs either at the sides or at the back. The economy of the boiler in first cost is obvious, and with proper proportions between the fire-grate and heating surfaces, as high an evaporative efficiency may be had as with most other forms of boilers. The quantity of water carried being comparatively small, steam may be raised with a small quantity of fuel and in a short space of time. Water may be left standing in the boiler for almost any length of time without injury. Every part of the boiler is at all times under ready observation, without disturbing the connexions, and the spheres may be easily swept on their outsides. The setting of the boiler is such also that the steam may be dried to any extent desired in the spheres themselves, without any other provision for superheating. It is thought that this boiler especially meets the increasing tendency to use high pressure steam, and that the description now given will, therefore, prove interesting to the members of this Institution.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, November 24, 1863.]

From the London Mechanics' Magazine, Nov., 1863.

During the past month there have been examined 238 engines and 381 boilers. Of the latter, 17 have been examined specially, 9 internally, 55 thoroughly, and 300 externally, in addition to which 1 of these boilers has been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture 10 (1 dangerous); corrosion, 25 (5 dangerous); safety-valves out of order, 1; water-gauges ditto, 30 (3 dangerous); pressure-gauges ditto, 5; feed apparatus ditto, 2; blow-out apparatus ditto, 15 (1 dangerous); fusible plugs ditto, 3; furnaces out of shape, 12 (3 dangerous); over-pressure 3 (1 dangerous); Total 106 (14 dangerous). Boilers without glass water-gauges 3; without pressure-gauges, 45; without blow-out apparatus, 48; without back-pressure valves, 72.

An illustration has lately been met with of the importance of removing portions of mid-feather walls, in order to give an opportunity of examining the plates. On this being done at the instance of this Association, in the case of a boiler lately put under its care, the bottom, although presumed by its owner and engineer to be perfectly sound, was found to be nearly eaten through by corrosion, and on the very point of rupture, as in the case of No. 39 Explosion, the details of which are given below.

Instances continue to be met with of serious corrosion, arising from the leakage of bolted joints concealed under brickwork. It cannot be too often repeated that all connexions to boilers should be made by means of fitting blocks riveted to the shell, excepting only the attachments to the front end plate, where they are not absolutely necessary, since the plate being flat the joints are more easily made, while at the same time, from their position, leakage, should it occur, is at once apparent. The front end plate should be left completely open, and not, as is too frequently the case, covered in with a wall of brickwork, since leakage—so prone to occur in the neighborhood of the furnace mouths—is found to go on behind the brickwork undetected. A case of this sort has just been met with, where a plate was completely eaten through before the corrosion was known.

Explosions.—No. 31 Explosion occurred at a distance of nearly three hundred miles, and as full details could not then be obtained, the fact of its occurrence was merely entered at the time in the tabular statement. The boiler was not under the inspection of this Association, and it has since been ascertained that it was of plain Cornish construction, having a single flue parallel throughout; its length being 25 feet 3 inches, its diameter in the shell 5 feet 7 inches, and in the flue 3 feet 5 inches, while the thickness of the plates was three-eighths of an inch, and the pressure to which the safety-valves were loaded, 40 lbs. Such a flue, on account of its large diameter, was unsafe from the day on which the boiler was made, though it might easily have been made secure by adopting any of the well-known plans of strengthening flues, among which may be mentioned, hoops, whether of T-iron, angle iron, or bridge rail section, as well as flanged seams.

Six explosions have occurred during the past month, resulting in the death of eleven persons, and serious injury to eighteen others. Not one of the boilers in question was under the inspection of this Association. One of these exploded boilers, No. 39, has been personally examined, while this was prevented in other cases by distance, some of the explosions occurring upwards of two hundred miles from Manchester. It is expected, however, that detailed particulars of all of them will be obtained, while I am already indebted for a minute report on Explosion No. 38, to the kindness of an engineer residing in its locality, and who made an examination shortly after the occurrence.

No. 38 Explosion, from which one man was killed, occurred to a boiler not under the inspection of this Association, and which was in use at an iron works.

The boiler was of a plain cylindrical egg-ended shape, externally-fired, and connected by its steam pipe to two others of different construction; all three collectively working an engine and forge hammer in the rolling mill. The ordinary working pressure of the boiler was 40 lbs., its length 35 ft., and its diameter 6 ft. 5 ins., while the plates were of the most unusual and unnecessary thickness, of from nine-sixteenths to five-eighths of an inch.

It is more difficult to make good work in a boiler with such thick plates, and it is reported that the joints in the present instance were imperfectly closed—that there had been unfair use of the drift, than which few things are more prejudicial to the strength of the boiler—that but few of the rivets were properly laid up, while, in addition, the overlap of the plates at the seams of rivets were unusually wide, and, in short, that the work was altogether most clumsy and inferior. Added to this, the boiler had been repaired over the fire more than once, with plates $\frac{1}{8}$ of an inch thinner than the rest, while longitudinal seams of rivets, six feet long, were allowed to fall within the furnace, their original length having been increased by the repairs.

The boiler had been leaking at the seams in the furnace for three weeks previous to the explosion; and in order to stop this the fireman had been in the habit of putting a bucket-full of sharps as well as a bucket-full of horse manure into the boiler once a week.

The state of the boiler after the explosion afforded no indications either of overheating from shortness of water or of corrosive action, while there appeared to have been little or no incrustation; but it was found that the shell had rent at one of the longitudinal seams over the fire, just where the thick old plates had been patched with the comparatively thin new ones. The rent extended throughout the whole length of the seam, and then ran into the transverse ones which crossed it at both ends, completely severing from the remainder of the shell the dome or egg-end. The latter was blown to a considerable distance, while the former, though lifted off its seat, was only thrown a few feet from it.

The cause of the explosion was made a subject of scientific investigation at the order of the coroner, and the view given in the report thereon by the engineer engaged, may be briefly stated as follows:—

The explosion was considered to be due to excessive pressure of steam, a pressure greater than that at which the boiler was ordinarily worked, and which it was thought might have arisen in the following way:—The explosion occurred shortly before four o'clock in the afternoon, the boiler having been cleaned out the same morning, while the other two to which it was connected were working alongside meantime. In order to clean this boiler out while the others connected to it had their steam up, it was necessary that the junction valve should be closed, and if this valve had not been re-opened, as it ought to have been, as soon as the steam was up—then in the event of the only safety valve with which this boiler was fitted, either sticking fast in its seat or being tampered with—the steam would be bottled up, and the pressure continue to rise, without giving any sign until actual explosion resulted, since there was no steam-gauge upon the boiler.

One or the other of these two conditions it was concluded had occurred viz:—either that the safety-valve had stuck fast in its seat, or had been intentionally tampered with, at the same time that the boiler was isolated from the other two by the junction valve being shut down.

The supposition of the junction valve having been left closed, was, it was thought, strengthened by the fact that the steam had been so low a few minutes previous to the explosion, that both engines had to be stopped, so that had the boiler in question been then in connexion with the others, it was argued that the rupture must have taken place when the pressure was lower than usual, which was not thought likely. That the safety-valve had stuck fast in its seat, was considered most improbable, while the supposition of its having been intentionally tampered with derives support from the reckless character of the fireman, who, shortly after the explosion had occurred, was known, while tending the boiler alongside the exploded one, to have gagged his alarm whistle on it signalling shortness of water; in consequence of which a number of workmen engaged near the boiler deserted their post stating that they considered their lives in danger.

The jury dissented from these views, and brought in a verdict of accidental death, adding—"That in their opinion the boiler did not explode from excess of steam, but from a combination of gases occasioned by the lifting of the junction-valve when there was not a sufficient pressure of steam. The consequence of the junction-valve being opened under these circumstances, would be, that the steam would rush into the boiler from the adjoining ones, and, by generating gases within it, blow it up.

The opinion of the jury can scarcely be regarded as otherwise than hypothetical, and as being entirely unsupported by experience.

The view that the boiler exploded from excessive and unusual pressure, consequent upon the safety-valve and junction-valve being both closed at the same time, is thought to be an unnecessary supposition.

considering the workmanship of the boiler, as well as its condition and the treatment it received, all of which have been previously described. A boiler, roughly made as this was, of plates of excessive and varying thickness, having an unusually wide overlap, and being in a leaking condition, with its seams choked up with bran and horse dung, such a boiler when exposed to the action of an external fire would be utterly untrustworthy, and the mere pressure of steam by no means the only element of weakness. Such thick plates, unless most kindly laid together, would be fighting one against another, especially under the action of the fire, when unequal expansion, aggravated by the varying thickness of the metal, would be sure to ensue. Moreover, had the pressure in the boiler at the time of explosion been higher than that at which it ordinarily worked, or indeed equal to it, viz: 40 ℔s., the main portion of the shell would never have remained so near to its original seating as it did, and thus the position of the fragments can only be accounted for by the supposition that the steam was low at the time of explosion, which is borne out by the fact that there was not power enough to drive the engines, and they were stopped in consequence.

This explosion, therefore, can only be regarded as an additional illustration of the dangerous character of the externally-fired boiler, especially when improperly repaired and carelessly attended to. The constant allusion to such cases may be tedious; but the subject is only recurred to on the repetition of explosion; and it is trusted that the persevering statement of these facts will assist in removing from general use so dangerous a class of boiler as those externally-fired have proved themselves to be.

No. 39 Explosion, which was attended with the death of one person and injury to four others, occurred to a boiler working at a cotton mill, and not under the inspection of this Association.

The boiler which was fifteen years old, was of plain double-flued construction, internally-fired, and of the class termed Lancashire, its length being 32 feet, its diameter 8 feet in the shell, and 3 feet in the flues. It worked alongside of another boiler very similar to itself, and to which it was connected.

The boiler had rent at the bottom of the shell, immediately over the mid-feather wall on which it had rested. This rent, which may be termed the primary one, extending longitudinally for about two rings of plates, and then assumed a transverse direction, running completely round the boiler and severing an entire, though rather irregularly-shaped, belt from the shell. The front end plate was separated from the other part of the boiler, while the furnace tubes, which appeared perfectly sound remained attached to the rest of the shell, which had been thrown out of its original seat and turned bottom upwards. The longitudinal stays for stiffening the end plates, though massive, were curled up into a scroll, and the transverse ones, which should never be introduced in a cylindrical boiler, proved, as might have been expected of no assistance in strengthening the shell.

The damage to the surrounding property was serious, a considerable portion of the mill being unroofed, and one of the side-walls blown down, while the boiler-house was reduced to a heap of ruins.

As to the cause of the explosion there can be no question; there was no evidence of shortness of water, the furnace crowns being uninjured; but on examining the edges of the plates at the primary rent, which had occurred over the mid-feather wall, their thickness was found to have been so reduced by external corrosion as not to exceed that of a half-penny piece.

In the scientific evidence given at the inquest, the plan of setting boilers upon mid-feather walls was strongly condemned, and the corrosion so frequently induced, and at the same time treacherously concealed by them, clearly pointed out. Of the truth of this there can be no question; and it is difficult to say anything more upon the danger of these mid-feathers than has already been said in previous reports while the occurrence of the explosion in the present instance affords an additional illustration of the correctness of the views so frequently expressed.

The jury considered that this explosion would have been prevented by due precaution on the part of the owner, and therefore brought him in guilty of manslaughter.

The Production of Sulphur in Italy. By M. P. Bianchi.

From the London Chemical News, No. 225.

The sulphur at present produced in Italy amounts to no less than 300,000 tons a year, the value of which in the rough state is 30,000,000 francs. This yield, which has increased ten-fold since 1830, is furnished in great part by Sicily. The quantity produced in Romagna, formerly but small, has since increased to 8000 tons per annum.

During the last ten years great improvements have been introduced in the method of extracting sulphur from its calcareous gangue. It is always obtained by liquefaction by burning a portion of the ore: but this operation formerly performed in small, open, cylindrical furnaces (*calcarelle*) is now effected by simply heaping the stones and covering them with earth as in charcoal burning. These heaps, called *calcaroni*, are of considerable size, often four hundred times larger than the old furnaces. This new mode of operating has the advantage of diminishing the losses occasioned by the production of sulphurous acid, so that the yield of sulphur is increased by one-fifth; besides sulphur can be burnt in this way near houses and gardens, which, with the old method was out of the question. Formerly it was burnt only at certain periods of the year, now it can be burnt at any time, so that it is no longer necessary to accumulate large quantities of ore. Finally, the operation, which used to be very frequently fatal to the workmen, is now almost harmless.

Sulphur of Romagna and the Marshes.—At Bologna there is a society called "Société des Mines de Soufre des Romagnes," which possesses eight mines, five in the province of Forli (Romagna) called Firmignano, Luzzena, Fosco, Busca, and Montemamro. The other three, forming part of the province of Urbino and Pesaro, are those of Perticara, Marazzana, and Montecchio.

Most of the sulphur from these workings is refined at Rimini, whence it is carried to the places where it is most in demand, such as Venice, Trieste, Ancona, Lombardy, Tuscany, &c.

This refined sulphur is chiefly used for making sulphuric acid, and lately for the treatment of the vine. Its price, which varies considerably, is in cakes, from 213fr. 10c. per English ton of 1·015 killogrammes, and in sticks from 254fr. 35c., put on board vessels in the ports of Rimini and Cesenatico, and delivered at the stations of Rimini and Cesana.

Sulphur from the Neapolitan Provinces.—Sulphur is found here in several places, but in small quantities. It is thus found in the volcanic region of Solfatares, where it exists mixed with clay and other matters from which it is separated by sublimation, but the yield is insignificant. Small deposits of it are found scattered in the district of Majella, one of which is worked at Santa-Liberata. It has recently been announced that there has been discovered at Civita-Nova a bearing of calcaire impregnated with sulphur, but nothing has been said as to its richness and extent. No more is known of another bearing at Santa-Regina, two miles east of Ariano.

Sulphur of Sicily.—Sulphur exists here in a gypseous bed, layers of which extend over a small portion of the island, from Mount Etna to near Trapani. This formation belongs to a geological epoch which has not yet been positively determined. Here, as in Romagna, it contains, besides gypsum, calcaires and clays, more or less marl. In the first case, the sulphur exists in a state of mixture, sometimes uniformly, sometimes irregularly, sometimes in small parallel veins and more rarely in the form of crystals; in the latter case it is not unusual to find it associated with *cœlestine*, or sulphate of strontium. In clay, on the contrary, it is found in globular masses which is also the case in similar bearings in Continental Italy.

There are about fifty mines in Sicily, employing 20,000 workmen. The most productive mines are in the provinces of Caltanisetta and Girgenti; ranging next in importance, are those of the provinces of Catana, Palermo and Trapani. The sulphur is extracted in the manner above described by means of *calcaroni;* the loss during the operation amounts to one-third of the ore. Most of the sulphur is exported in the crude state, but little being refined in the island. In this state it is divided into three qualities, the second and third being subdivided into three other qualities. The yield in 1861 was estimated at about 240,000 tons of commercial sulphur, of which about half was produced by the province of Caltanisetta, a third by Girgenti, 25,000 by Catana, and 20,000 by Palermo; the quantity produced by the province of Trapani is very inconsiderable. Most of the sulphur is exported to France and England.

The price of this product has risen during the last few years; in 1860 it sold in the crude for, from 15 to 20 frs. the ton.—*Moniteur Scientifique*, v. 799. 63.

Apparatus for the Prevention of Steam Boiler Explosions and Railway Accidents.

[From the Proceedings of the London Association of Foremen Engineers.]

From the Lond. Mechanics' Magazine, Sept., 1863.

The Chairman called upon Mr. Gettliffe to read his promised paper on "an Apparatus for the prevention of Steam Boiler Explosions and Railway Accidents." That gentleman, after some preliminary observations, proceeded to say that the apparatus in question was the invention of M. Auguste Achard, C. E., of Paris, and that it was known in France under the title of the "Embrayage Electrique." It was calculated to maintain a constant level in steam boilers by automatic means, and without the assistance or supervision of the engine-man. This regulator, as it might be termed, consisted of a ratchet wheel of peculiar construction, and which was keyed, upon a spindle turning freely in its bearings. Right and left of the wheel were mounted, loosely on the spindle, two wrought iron levers of about 20 ins. each in length, and connected together by cross-pieces. A two-armed "click" supported by the levers, turned freely round an axis. The double click acted freely upon the ratchet wheel which was furnished with two sets of teeth, turned in opposite directions to each other, and separated by a blank space. The click was fitted with a tail-piece in the form of a fork, the two prongs of which were braced together by a cross rod. In the fork was mounted an "armature," which was provided with an opening through which passed the cross piece. The armature was made of brass, and to it was attached a plate of soft iron fitted in so as to be flush with the surface of the armature. Opposite to this latter was placed a horizontal electro-magnet, the poles of which abutted against the armature in such a way as that it (the armature) could only slide against the poles without coming into immediate contact therewith. The part played by the armature was merely to act as a weight on the levers supporting the fork; for it would be understood that when the electric current was in circulation, the armature would be suspended by its adherence to the electro-magnet, and thus relieve the fork of its weight. The suspension of the current, on the contrary, would cause it to drop on the cross-piece connecting the levers, and thus alter the gravity of the fork. It was these alternations of the weight on the tail-piece of the levers which caused the fork to act upon the upper or lower set of teeth on the ratchet wheel; or in other words, to open or shut the feed-cock of the boiler. It was necessary that the fork should have continual motion—a kind of pump-handle movement indeed—which could be effected by a simple contrivance (a small eccentric, for instance, attached to a running shaft on the general machinery). The effect would be that the compound ratchet wheel would be acted upon and moved in one direction when the boiler required feeding, and in the other when it had been fed sufficiently. If all were going on well, the click would be simply moved over the blank portion of the wheel, and communicate no motion whatever to it. Proceeding now to the general action of the contrivance, the reader proceeded to state that when the current was broken off from the electro-magnet, the weight of the armature operating upon the cross-piece would press down the tail-piece of the levers carrying the click, which latter would rise and act with its lower arm upon the ratchet wheel. The consequence would be, that the wheel would turn from left to right to the extent of one tooth, or about half an inch, at each oscillation of the levers, until the blank space presented itself and stopped further movement. This would be at a point when the wheel had made one-fourth of a revolution. On the other hand, when the current was in circulation, the armature in sliding against the poles of the electro-magnet, would bring the plate of soft iron into contact with the poles, which would then hold it firmly, and prevent the armature descending with the click during the downward oscillation of the levers. The tail-piece would thus be relieved, and not have to support any weight while the levers rocked in the opposite direction. It would follow that the upper arm would then act upon the wheel and cause it to turn the reverse way, *i.e.*, from right to left. It would thus become evident that the two-armed click would act on the ratchet wheel, and cause it to move in opposite directions, whether the electric current were traversing the electro-magnet or not. "Now," said the reader, "the characteristic feature of the electric regulating apparatus is arrived at." The rising or falling of the float in the boilers of stationary or land engines controls the electric current, and causes it to circulate or be interrupted at the proper times. This result is obtained directly by the wires which proceed from the pile or battery being made to pass first to the float instead of being connected immediately to the electro-magnet. The float itself was connected by a rod with an index, on the lever of which was mounted a piece of wood faced on one side with a brass plate. On another piece of wood, in the form of a rocking lever, and moving on an axis, was a second plate of brass. The electric wire from one of the poles of the battery was fixed to the first named plate, and the other end of the wire, after traversing the electro-magnet, was attached to the other pole of the battery. In order that the current might circulate, it was indispensable that both plates should meet, and from the peculiar arrangement of the two metallic plates, they would always be in contact when the index was at zero (the normal feeding point) or above it, but the contact ceased as soon as the index fell below zero. Let it be supposed that the feed-cock of the force-pump, or other contrivance for supplying water to the boiler, was connected to the spindle of the ratchet wheel: it would follow that the cock must partake of the movement of the wheel, and that it will make a quarter of a turn in one direction when the electric current circulated, and a quarter of a turn in the other when the current was interrupted; thus the automaton regulator, which was sensitive to the smallest alteration in the water level of the boiler, was perpetually and inevitably maintaining it at a constant height, and thus one great scource of danger was obviated. As an extra security, however, against accident by explosion, a minor contrivance was affixed to the same apparatus, and which owed its action to similar principles. On the side of one of the levers was fastened a cap with a gudgeon in its centre. An armature resembling that already described was carried by this cap and rested on the gudgeon. In connexion with the second armature was placed an electro-magnet, against which it was made to slide and press constantly upon the poles. It was still the index of the water gauge, which induced the circulation or effected the interruption of the electric current; and the same electric pile or battery, composed of a single element, or Daniel's pair, sufficed to put in motion the two electro-magnets. The electric wire which proceeded from one of the poles of the battery, and was affixed to the brass plate on the lever of the index, served for the two electro-magnets also. The returning wire corresponding with that about to be described, however, was fixed to a third brass plate, and, after traversing the electro-magnet was attached to the second pole of the battery. The last mentioned plate was arranged in such a manner as to establish a metallic contact, which permitted the electric current to pass, notwithstanding the variation in the level of the water to the extent of an inch above or below its proper line. If the variation exceeded these limits, the current would be forcibly broken. When the electric current was in circulation—that is to say, when the lever was between the limits of variation named—the armature would be suspended by the adhesion of the second electro-magnet; but if the level rose or fell beyond those points the armature, from the current being broken, must fall. In either case, a small tappet fixed to the armature below the gudgeon touches an alarm lever and thus rings a bell. This arrangement was also serviceable in other respects. The alarm bell rings when the feed pump is out of order; when the electric pile ceases to act; if the stoker neglects the fire; or if steam is generated too rapidly. The reader proceeded to point out the application of the "Embrayage Electrique" to the prevention of railway accidents by connecting it with brakes upon the wheels of the engines and carriages, and certainly gave some very practical illustrations of its value and efficacy in this respect. In France it is extensively used, and testimonials in its favor from many eminent scientific men were submitted to the Associated Foremen Engineers on Saturday night. Mr. Gettliffe, moreover, exhibited the apparatus itself to his audience, and thus gave them a further opportunity of judging of its characteristics.

On the conclusion of the paper, a short discussion arose: and in this Messrs. M. Jones, Ives, Briggs, and Bragg took part. In reply to questions put by these gentlemen, the reader stated that at the works of Mr. Carter, engineer, &c., Grove, Southwark, an apparatus of the kind he had attempted to describe might be seen in daily action, as also at Messrs. Simpson's, in Little Britain City. The office of M. Achard was at 1 Little Carter-lane, St. Paul's.

The Chairman then proposed a vote of thanks to Mr. Gettliffe for the able way in which he had explained an ingenious apparatus of great interest to the members of the Society of Foremen Engineers. He had, from what he had heard and seen on that evening, formed a good opinion of the contrivance, and he should take an early opportunity of witnessing its action. The time was approaching when Electricity would become a universal handmaiden to applied science, and mechanical men would do well to study closely its phenomena. M. Achard had, in the production of his "Embrayage Electrique"—in substitution for the title of which an English one must be found as expressive—demonstrated his great talent, and in Mr. Gettliffe he had found an excellent expositor.—The vote of thanks having passed, the meeting shortly after separated.

A Critical and Historical Review of Locomotive Engineering.

By J. J. Birckel.

From the London Artizan, Jan., 1864.

In our paper of January, 1863, we have shown that the total quantity of heat contained in a given weight of steam remains practically constant for all pressures, and we have therefrom drawn the conclusion that steam engines become more economical gradually as the working pressure of the steam increased, without, however, giving any numerical value to the rate of increase of economy with pressure. As this subject cannot be too forcibly impressed upon the minds of our readers, having been enabled to supply this interesting item of information by means of the data on the density of steam furnished to us by Drs. Rankine and Fairbairn, which we have embodied into our paper of October, 1863, we now give the same in the table below, and the mode of obtaining it is as follows:—

1st. For a given volume of steam the quantity of energy created varies directly as the pressure; 2d. The total heat contained in two equal volumes of steam of different pressures is proportional exactly to their respective densities; and the economic value or the absolute energy contained in steam is, therefore, represented by *the ratio* of the rates of increase of density with pressure.

With regard to the useful work obtained from steam, it may be assumed in this comparison that in non-condensing engines all steam admits of being expanded down to the pressure of the atmosphere, and that the whole of the energy contained in atmospheric steam is absorbed by the resistance of the atmosphere; upon these assumptions the quantity of work in foot-pounds contained in a pound weight of steam at any pressure is expressed by the formula (1),

$$p\,v\left(1+\text{hyp. log.}\frac{v_1}{v}\right)-p_1\,v_1 \qquad . \qquad . \qquad (1)$$

where p and v stand for the pressure per square foot, and the volume in cubic feet of a pound of steam of any density, and p_1 and v_1 for the pressure and volume of a pound of steam at the atmospheric pressure; the results of this formula are contained in column 5 of the table below, and the rate of increase of efficiency or the economic value of the steam is contained in column 6.

TABLE

OF ECONOMIC VALUE OF STEAM AT DIFFERENT PRESSURES.

Absolute Pressure in lbs.	Rate of Increase of Pressure.	Rate of Increase of Density.	Economic Value of Energy Created.	Non-condensing Engines.		Condensing Engines.		Rate of Economy, non-condensing Engines being taken a unit.
				Work in one Pound of Steam.	Economic Value of Steam.	Work in one pound of Steam.	Economic Value of Steam.	
15	1,000	1,000	1,000	0	$\frac{1}{\infty}$	48,296	0,54	∞
25	1,666	1,620	1,027	29,235	0,72	78,122	0,87	2,6
30	2,000	1,917	1,042	40,392	1,00	89,450	1,00	2,2
40	2,666	2,533	1,053	57,674	1,42	106,914	1,19	1,8
50	3,333	3,090	1,078	72,634	1,79	122,424	1,36	1,6
60	4,000	3,682	1,086	84,065	2,08	133,990	1,49	1,5
70	4,666	4,274	1,091	93,922	2,32	143,901	1,60	1,5
80	5,333	4,850	1,104	103,518	2,56	153,731	1,71	1,4
90	6,000	5,102	1,110	111,140	2,75	. .	. .	. .
100	6,666	5,922	1,112	117,746	2,91	. .	. .	. .
110	7,333	6,574	1,117	124,066	3,07	. .	. .	. .
120	8,000	7,151	1,122	129,733	3,21	. .	. .	. .
130	8,666	7,660	1,130	136,226	3,37	. .	. .	. .
140	9,333	8,202	1,137	141,742	3,50	. .	. .	. .
150	10,000	8,731	1,143	147,004	3,63	. .	. .	. .
160	10,666	9,282	1,148	151,662	3,75	. .	. .	. .
165	11,000	9,553	1,151	153,902	3,81	. .	. .	. .

It may not be without interest, while engaged upon this subject, to extend this comparison to condensing engines, in the case of which it may be assumed that steam at any pressure can be expanded down to 5 lbs. below the atmosphere, and that one-half the energy contained in steam at the atmospheric pressure is absorbed by the air-pump and the condenser, though in practice it never reaches that amount; the work in foot pounds contained in a pound of steam, at any pressure, is then expressed by formula (2),

$$p\,v\left(1 + \text{hyp. log.}\frac{v_2}{v}\right) - \frac{1}{2}p_1\,v_1 \qquad . \qquad . \qquad (2)$$

where $p\ v$ and $p_1\ v_1$ retain their former meaning, and v_2 represents the volume in cubic feet of a pound of steam at 5 lbs. below the atmospheric pressure; the results of this formula are contained in column 7, and the rate of economy in column 8. For the information of those who are of opinion that the condenser has become a useless adjunct to the steam engine, and should be consigned to its grave alongside the immortal Watt, its father, we have given in column 9 the relative economy of condensing engines compared with non-condensing engines, and we shall be much surprised if these figures do not cure those who labor under the above-named delusion, should they cast their eyes upon these pages; these figures, in fact, show that at any pressure condensing engines must be more economical than non-condensing engines working at the same pressure; and from columns 7 and 5, it may be seen that steam at 55 lbs. effective pressure in condensing engines yields as high a duty as steam at 150 lbs. effective pressure in non-condensing engines; and this, no doubt, does account for the fact that the locomotive engine, though working under great disadvantages, such as high velocities of piston, contracted and difficult steam passages, cooling influence of the atmosphere, and others, works nearly as economically now as the marine engine or even the stationary engine.

It is to be regretted, however, that superficial observers, instead of tracing this latter fact to its real source, have drawn from it the conclusion that very high speeds of piston are not detrimental to the economic working of the steam engine, and are advocating high piston speeds in consequence; although all observations with the indicator show, and scientific reasoning sustains the fact, that steam working under such conditions loses a great per centage of its energy through wire drawing and friction.

We are fully aware that the quantities of work given in columns 5 and 7, are not actually realized, but the ratios given in columns 6 and 8 are not on that account considerably altered. We know also that steam is not expanded down to the limits we have assumed, but the assumption which we have worked upon only shows in a less favorable light the one fact which we have desired to impress upon the minds of our readers, viz: that the steam engine becomes economical in proportion as high pressure steam is used.

Prevention of Decay and Oxidation in Ships.

From the Journal of the Society of Arts, No. 572.

As the prevention of decay in the timbers of wooden-built ships, and the fouling and oxidation of the plates of iron ships, have received considerable attention from the Admirality and Merchant Ship-owners, the following *resumé* of all the specifications at the Patent-Office bearing on this subject is given:—

Dipping timber in boiled oil is a very ancient practice, and it would be difficult to trace its origin. In 1739 Alexander Emerton took out the first recorded patent for preserving wood from decay. He prepared the planks or boards with boiling oil in the then old way, and afterwards coated them with compounded poisons, powdered glass, and sand, cemented with painting colors and oils, laid on as paint. The next patent, which was for preserving copper, or plates of which copper is a basis, was granted in 1790, to Collins & Wyatt. They covered the plates with lead or tin. In the earlier part of the present century several chemists recommended decoctions, in the form of vegetable poisons, for saturating timber, and thus destroying all animal life in the green wood. None of these poisonous solutions seem to have succeeded, for, had they been found efficacious, their application would have been continued. There was then an interval, during which the stoppage of decay seems to have been abandoned, and dry rot allowed to take its course. In 1822, John Oxford secured a patent, whereby he undertook to prevent oxidation or decay in iron or wood, by preparing tar in such a manner as to stop the evaporation of the oil contained therein, saturating it also with chlorine gas. This purified oil is then mixed with 100 parts of white lead—or of red oxide—25 parts of carbonate of lime, and 25 parts of purified coal tar added to the oil of tar. These ingredients are then applied as a paint. In the first days of iron in ship-building, rust was found to be a drawback to its general introduction. Scientific men saw the disadvantage, and sought to remedy the defect. Galvanic action, it was considered, would set all right, and the earliest patent in this direction was taken out by G. G. Bompass, in 1830. He sought to preserve metals from corrosion by an electric or galvanic process. For copper to be protected in sea-water he attaches an alloy of 9 to 10 parts of zinc with 100 parts of copper. In protecting iron he employed an alloy of tin consisting of from 10 to 150 parts of tin combined with one of zinc. Following in the footsteps of Mr. Bompass, in the same year, Mr. John Revere patented an invention for fixing zinc protectors to the brace or stud of chain cables, and other iron surfaces exposed to the action of salt water. These galvanic zinc protectors were riveted or soldered on according to requirement. It is known that one of the most distinguished of our electricians is in favor of inserting strips of zinc in the plates of ships; but if this principle proved correct in practice, it would long since have been universally adopted. Zinc plays an important part in patents for the prevention of oxidation. In 1837 Jacob Perkins got protection for a plan of coating copper tubes of boilers with a preparation consisting of two-thirds of zinc with one-third of copper; but he had been preceded in 1832 by Captain H. W. Craufurd, R.N., who proposed to preserve copper and iron from oxidation by coating with zinc paint in a fused state. Over this he laid a second covering of pure tin, or tin alloyed with lead. Captain Craufurd explains in detail his method of compounding the ingredients. In 1838, Le Comte de Fontainemoreau, considering Captain Craufurd's mode of fixing or adapting the zinc to this purpose as erroneous, applied for a patent of a more comprehensive description, for applying the zinc coating to metals. Again, in 1839, Mr. Thomas Dowling patented what he terms a conservative bath, applied to metals after grinding. He describes the machinery by which he effects this, the chief of which is a zinc wheel and galvanic vapor furnace.

In 1840, Mr. J. R. Neilson came forward with his invention for the application of a coating of copper, or copper alloyed with zinc or tin, or both, to the surface of iron. This was done by covering the mould to be cast with the material. In malleable iron, dried borax or flux is spread over the iron, which is then prepared with alloy heated to a temperature sufficient to melt copper, and, in its heated state, plunged into cold water to detach the scale of oxide. Mr. Arthur Wall, likewise in 1840, mixed 20 pounds of the strongest muriatic acid diluted with three gallons of water, then added 12 lbs. of steel or wrought iron filings. The filings were heated to redness before mixture. The whole was then subjected to heating in a pan, &c., and the composition was then applied to prevent corrosion. In 1841, Mr. W. E. Newton employed silicates of potash, or soda, for making a plaster or coating to prevent iron from becoming rusty. After him came Professor R. Mallet, Engineer, of Dublin, whose varied processes are of the most complex character. Finding that iron covered with zinc, when immersed in sea-water, and certain fresh waters, gathered to itself a coating of carbonate of lime, destructive to the protective power of the zinc, and affording a surface for the attachment and growth of marine animals of the molluscous and testaceous classes, and aquatic plants, he applied chemical means to detach the scales of oxide from iron, and then plunged it into a preparing bath. After undergoing a series of processes, the metal is coated with an alloy or zoofagous paint, which paint is rendered poisonous by admixture of salts of metals, by means of which he sought to render the zinc effective as an anti-corrosive protector. In 1841, also, Mr. E. Morewood endeavored to preserve iron from oxidation or rust by tinning it, and then dipping the tin covering or surface in molten zinc. Moses Poole, in 1845, claimed to possess an invention whereby he rendered iron more hard and durable, and free from oxidation. by the use of ferrocyanide of sodium, calcium, barium or any other alkali, or alkaline earthy base; to be used in a manner fully set forth in his specification. In 1846, Mr. Andrew Smith improved upon the plans for melting the zinc. He employed a bath of lead or tin, or any composition or medium that melts at a lower degree of heat than zinc, by which means the heat from the fire of the furnace s taken up and transmitted to the receptacle containing the zinc for melting. Baron Wetterstedt, in 1846, added the regulus of antimony to lead sheets, combined copper with antimony, made sheet metal by using lead and tin, and lastly, protected metals by paints thus prepared:—1st. One part of regulus of antimony to three parts of copper, mixed, melted together, run out into water, and then heated gently. Two parts of oxide of copper are added, and moistened with naphtha. The whole is then added to a composition of tar and naphtha. 2d. Another paint is compounded of 30 lbs. of tar, 30 lbs. of pitch, 20 lbs. of dried soot, 4 lbs. of tallow from sperm oil, and naphtha added for consistency.

Mr. C. H. Paris, in 1849, coated metals with glass or vitreous matter. The metal went through a cleansing process. Gum water is then applied, and over this the dry or powdered glass is shaken, and then fused by heat till a glass surface is formed. Mr. Paris claimed the application of carbonate of soda for applying glass in this manner. Mr. J. MacIntosh, in 1852, made a paint from decomposed india-rubber in combination with oils or fatty matter, saponified by metallic salts, with lime for thickening the liquid. For bottoms of ships he recommends the india-rubber, when in a fluid state, to be combined with metallic soap, thickened with lime and colored by pigments. Messrs. Hughes and Firmin, in 1852, manufactured lamp black from the vapor of coal tar, dead oil, dead oil salts, coal pitch, naphtha, linseed oil, and other materials. From the products a fuel is produced, and this residue has by another inventor been mixed with oils, ground and made into a paint. In 1852, also, Mr. R. M. Glover took out a patent for a preparation of arsenite of lead and arsenite of copper, and the red and yellow sulphurets of arsenic. The proportion of each were as follows:—Two parts, by weight, of arsenite of lead, one of realgar, one of orpiment, and one of arsenite of copper. In the same year, Mr. J. Murdock invented a variety of driers for zinc when the white of zinc is employed instead of white lead. The protoxides are manganese, cobalt, iron, tin, and nickel; for acids, the benzoic, urobenzoic, and the boric. In 1852, Mr. Binks, patented a substitute for linseed or drying oils, in the products derived from dissolving what are called insoluble soaps. A pigment is then ground in this solution, and the paint is ready for application. The pigment may be white lead, oxide of zinc, lamp black, or any other. J. C. Medeiros, in 1853, proposed the use of mercury or quicksilver on iron plates for sheathing ships. The salts of mercury are dissolved, then a bath is formed and the plates allowed to remain in the solution till their surface is equally and regularly amalgamated.

Mr. Newton, in 1854, made a paint from ground plumbago, pulverized charcoal, and the black soot formed by the burning of bituminous matter, along with ivory black, or bone or lampblack. Mr. Ryder also, in 1854, described a method for mixing gutta-percha with common resin, or tar, pitch, or asphaltum, dissolving them in impure benzole or coal naphtha. Mr. Newton took out a second patent, in 1854, for the production of a siccative black, brown, or grey pigment or coloring matter, by the admixture with the gas tar, or other organic substance to be carbonized for the purpose, of the oxides of potassium, sodium, calcium, aluminium or other alkaline or earthy bases for paints. Mr. F. Ransome, in 1854, patented a mixture, consisting of ground oxides and carbonates of lead or zinc, and carbonate or sulphate of barytes with soluble silica. Mr. J. Rogers, in 1855, to prevent oxidation, deoxidizes metallic ores by a revolving cylinder, fitted with helical or screw-formed divisions to receive the ores in a pulverized state, and then submits the same to heat, and constant agitation by the revolution of the cylinder. Mr. B. Rosenburg, in 1855, manufactured a paint as follows:—100 lbs. of triturated white lead, $2\frac{1}{4}$ gallons of copal varnish, $1\frac{1}{8}$ gallons of spirits of turpentine, $1\frac{1}{8}$ gallons of linseed oil, and, for coloring, a small quantity of red lead. Before the metal is painted it is subjected to the fire for cleansing, and when cool the preparation is applied, then varnished with copal, and dried by a hot air process. Mr. J. E. Cook, in 1855, proposed a composition consisting of gum shellac, dissolved in methylated spirit or in wood spirit. In 1856, the patent of Messrs. Bancroft & White claims the manufacture of oils from petroleum, for preserving metals and ships' sheathing. Mr. A. F. Mennons, in 1856, obtained a patent for a non-conducting and inoxidable composition for metals, made thus:—

Argillaceous clays, containing a certain proportion of alumina,	100	parts.
Oily substances and residues, . . .	6	"
Oil sediment, ...	5	"
Fat, ...	2	"
Animal charcoal, ...	2	"
Mucilaginous substances, such as glue, &c., . .	2	"
Wood sawdust, already employed in the purification of oils in the process of dyeing, ...	10	"
Waste hair well beaten, ...	4	"

To the preceding materials a decoction of logwood and soot is then added.

Mr. J. M'Innes, in 1856, was granted a patent for coating metals with powdered emery stone mixed with a varnish of shellac dissolved in spirits of wine, with the addition of castor oil. As emery contains 87 per cent. of aluminium, Mr. M'Innes considered that this paint would be solid enough to resist all action in the water, and never decompose. Mr. R. D. Atkinson, of Hull, in 1856, invented a plan for coating and protecting metals from oxidation, by depositing copper or brass upon surfaces of prepared iron, the deposit to be melted in conjunction with carbonic acid gas, the coating to be put on by a brush, or through the medium of galvanism. Depositing brass on iron is now being successfully carried out at Portsmouth, by Mr. Wielan, on armor plates and other iron surfaces. Mr. A. Reid, Mineralogist, in 1856, describes in his specification what he deems a sure way of preventing oxidation. He places the iron in a properly constructed furnace, then covers the metal with soot, or other matters possessing the like element, the temperature is then raised to red or white heat, and continued for half an hour, or according to the size of the iron operated upon. It is then suffered to cool, the surface is cleaned, and Mr. Reid asserts that a coat impervious to rust is formed. If this is verified by positive experiments, the cheapness and simplicity of the plan deserves to be widely known. Mr. Joseph Poleux, of New York, communicates, in the same year, a plan to overcome oxidation. He employs muriatic acid, nitric, or sulphine acid, of the ordinary degrees of concentration in commerce. without dilution combined with the introduction of spelter into the cleansing process. In 1857, Mr. G. Bedson patented a new process. He melts a quantity of pitch derived from mineral tar, and a proportion of tar oil, with caoutchouc tempered with tar oil and shellac, the substance to be solid and elastic when cold. Mr. C. F. L. Oudry claims depositing copper on a preservative or intermediate coating instead of on the metal. He deposits copper in a pure state to any thickness on all metals. Mr. C. Iles, in 1857, describes a means of applying earthy cements to metals. In 1858, Mr. J. Coutts received a patent for applying the following pigment by heated air:—

Carbonate of baryta, . . .	·650
Litharge, . . .	·065
Arsenious acid, . . .	·050
Asphaltum, . . .	·050
Oxide of calcium, . . .	·010
Creosote (oil of tar), . . .	·175

Perhaps the most novel introduction is that patented here by Messrs. Bouchard & Clavel, the Paris bankers, in 1858. On the estate of La Gruerié, in Charney, Department of Yonne, France, is found an earth of the ochre description, called "Burgundy Red." This earth contains most valuable properties, and is said to be an exceedingly good preservative against rust. It is used as a cement and paint by admixture with the following:—

Burgundy red, . . .	66 parts,
Grease of oil, . . .	15 "
Lime, ...	11 "
Unburnt earthenware, chalk, or Roman cement,	8 "
	100 "

This is said to prevent oxidation if the earth is mearly diluted with volatile oil. D. M'Crae, in 1858, was allowed a patent for preserving bottoms of ships from fouling or decay. He applies grease from the cells of boiled bones, kitchen-stuff, and butter without salt; a poisonous matter is mixed with these fatty substances. Mr. G. P. Lock, in 1858, made a composition for the under coating of iron ships, made from iron ore ground in boiled linseed oil 50 per cent., oil of turpentine 50 per cent., well mixed. For the outer coatings, white lead 40 per cent., blue mineral or copperas 10 per cent., and oil of turpentine 50 per cent. In 1859, Mr. Henry, on the part of Moisant & Co., sought protection for bituminous products and compounds of bitumen for preventing oxidation. Mr. T. J. Laballe made a preparation of caoutchouc paints and colors for vulcanizing. Mr. J. Crawford, of Liverpool, in 1859, applied for a patent for a metallic paint or varnish, composed of plumbago, or black lead, fine or gum varnish, arsenic, and spirits of turpentine mixed. Mr. F. W. Emerson, in 1859, prepared an anti-corrosive paint from oxi-chloride of lead, mixed and ground with oil, turpentine, varnish, or other vehicle. Mr. Weild, in 1859, sought to economize time and labor, by a mechanical machine for applying paints to metals on large surfaces. Mr. James Meikle, in 1859, proposed coating iron ships with asphalte. In the same year M. Auguste Pin dissolved sugar in muriate of zinc, then added wax and soap, in which was incorporated calcareous stones, phosphate of soda, sulphate of zinc, and copper, and the syrup of potatoes or sugar, with powdered marble, quartz, or felspar.

In 1859, Mr. F. G. Spilsbury, of Louvain, applied for a patent for the manufacture of a paint. He took sulphate of lead, and heated it to a red heat, either by itself or mixed with alumina or other earths; the pigment thus obtained to be washed first with sulphuric acid, then with water, when it is finally dried. Previous to drying the pigments are digested with salts of tungstic acid, molybdic acid, titanic acid, tantalic acid, arsenic acid, acid of antimony, or other metallic acid, or with mixture of the above salts. A combination between the sulphate of lead and the metallic acid or acids is obtained, and the resulting pigments are dried in like manner after having been cleared from all adhering salts. The pigments may then be mixed with oil and used as a substitute for white lead. Mr. J. F. J. Lecocq, in 1860, prepared a calcareous varnish for coating iron and the bottoms of ships. Mr. H. Kemp, in 1860, patented a composition consisting of peat tar, wood tar, methylated spirit, peat oil, or linseed oil, arsenic resin and carburet of iron, for preserving ships bottoms. Mr. Allen's plan for making a coating or anti-corrosive paint for metals is thus given:—Ammoniacal liquid obtained from coal tar, or gas tar, prevents incrustation in boilers, and is applicable to painting the inside plates. Messrs. Pile and Smyth, of West Hartlepool, took out a patent in 1860. They employ a red composition and enamel, consisting of a combination of litharge, Venetian red, and pine varnish. Over this composition is applied a coating of resin. gums, or any pitch or bituminous substance with the addition of coal tar or oil. This is put on in a hot lava state, and the process is called enameling. An impermeable oil varnish was patented by M. Antoine Bonet, in 1860, composed of 100 parts of alcohol, 100 parts of spirits of turpentine, one part of sulphuric ether, and one of carbonate of soda.

Mr. Robert Smith, ship-owner, of Finsbury, applied to the Patent-Office, in 1860, to protect his system for keeping vessels from fouling and worming. He applies equal parts of pitch, tar, resin, and turpentine, with any other adhesive compound. Assafœtida to be mixed with the foregoing, as a poison to destroy life. When the coating is laid on, and dry, the whole to be covered with paper or cloth. Mr. G. Hallet, in 1860, in his patent explains his method of protecting metal. He grinds the oxide of antimony to powder, then dries it, and mixes with it 12 lbs. of linseed oil to the hundred-weight of powdered oxide. Mr. Richardson, 1861, to prevent oxidation, would cover the metal with vulcanized india-rubber, cloth, or gutta-percha, the object sought being to provide for unequal expansion of the metal and coating. Mr. Francis Pulz, Chemist, 1861, causes oxygen to be passed through sulphuric acid, to render the oxygen more active as an oxidizing agent, as it combines, when so treated, with other substances for which it has an affinity, for manufacturing purposes. Mr. Pulz, also, in a second patent, submits oily matters to this oxidizing agency, by causing the sulphureted gas to pass through them when in a liquid state. Mr. Martin Miller sends a communication, in 1861, for coating metals by metals or alloys in different ways. Mr. John Hay, in 1861, patented a drying oil. He lays a non-conducting coat, and then makes a paint by grinding in linseed oil the black or protoxide of copper, which is then boiled till reduced to the sub-oxide, and by thus oxygenating the oil he claims to have formed a quick drying cupreous oil. Mr. John Snider, of the United States, patented here a compound, in 1861, for coating metals. He reduces amorphous graphite to fine powder, and then mixes it with ore by the agency of a heated steam-pipe. When cool and dry, one pound of oil is added to three pounds of the powder, and when the ingredients are combined, hot pure bees-wax, in the proportion of one pound of wax to 10 lbs. of graphite, is mixed. Afterwards linseed oil may be added. Mr. Snider details his manner of manipulating and preparing the graphite and ore. Messrs. Hallett and Stenhouse, in 1861, obtained a patent for the manufacture of pigments for coating surfaces. They employ native oxide of antimony, chemically treated in ways too intricate for explanation in this abbreviated outline, and mixed with red lead or litharge. They sometimes take type metal or worn-out types, reduce them to a coarse powder, and then mix them with their own weight of zinc, and calcine them. This produces a yellow pigment.

New Engraving Processes,—Sunk and in Relief.

From the London Builder, No. 1103.

Some curious experiments, executed before the Emperor and Empress of the French, by M. Dulos, are thus described in *Galignani's Messenger*:—A sheet of copper, drawn on with lithographic ink, is subjected to the voltaic pile, by which it receives a deposit of iron on those parts not covered by the ink. The ink is then removed by means of benzine, and the sheet is then in the state of an engraved copperplate—that is, the design is sunk into the plate, and the deposit of iron constitutes the lights. The plate is now dipped into a bath of cyanide of silver: under the action of the galvanic current, silver is deposited on the copper, and not on the iron. The plate is then taken out, and mercury poured upon it. Now, since mercury exercises no action on iron, it can only attack the silver which has filled up the lines originally made with lithographic ink. Hence, where those lines are, we now see mercury standing up in relief. Plaster of Paris is now poured over the plate in this condition, and thereby an impression is obtained in which the reliefs made by the mercury are re-produced hollow. [Why the ink was removed, or silver deposited in this case, is not clearly stated.] By the aid of the pile copper may be deposited on this impression, and the hollows will then come out again in relief. One thus obtains not only a plate which may be printed from, but a matrix which will give an indefinite number of plates. For typographic engraving, the sheet of copper, after being drawn upon, receives a coating of silver, which only goes upon the metal, leaving the lithographic ink untouched. The ink is removed, as before, by benzine. The surfaces which are thus laid bare are oxidized, and mercury is again poured on the plate. This metal recoils from the oxidized lines, and collects on the silver, making reliefs where the lights ought to be. An impression in plaster is obtained as before, and the electro-chemical plate which is to be used for the printing has the lines of the drawing in relief as required.

For the Journal of the Franklin Institute.

Improvements in the Barometer and Thermometer. By JOHN THOMSON, of Wayne, Du Page County, Illinois.

The Committee on Publications will please insert in the "Journal of the Franklin Institute," this notice of a series of improvements on the Barometer, including also the Thermometer, which I shall detail as briefly and explicitly as possible.

First in order is what the inventor designates as the Farmer's barometer. It consists of the stem of a common *Thermometer* tube. Into this tube is introduced a single drop of mercury and then both ends of the tube are covered with a piece of silk. A metallic bulb is then cemented to one end of the tube, while the other is left exposed to the action of the atmosphere. A small steel ring is placed upon the stem to slide up or down; and the instrument is complete.

The object of the inventor in producing this instrument, was wholly to befriend the farming community, and such others as are more or less dependent on the condition of the weather, for their operations, by giving them a cheap barometer, as reliable to the most expensive instruments that are made.

It will be observed that the small globule of mercury within the tube is the index, whose position is to be noted, forming as it does a movable partition, dividing the air in the tube into an upper and lower portion. As changes in the weather are indicated not by the actual weight of the atmospheric column, but by changes in that weight, so this instrument is designed especially to indicate those changes, giving notice of the coming storm, or the approach of more favorable weather. No scale of inches is upon this instrument, as it is designated only to indicate *changes.*

The standard place for the index is about midway of the tube, when the temperature is 50° and the atmospheric pressure 30 inches.

To make an observation with this instrument, place it in a tumbler of newly drawn water from the well or spring, which may be supposed to be of the same temperature from time to time. The air in the bulb is thus brought to the same temperature as the water. Now upon the upper side of the index we have the free atmospheric pressure while upon the lower side presses the elastic force of the combined air; and the index itself, moving freely in the tube, will always occupy the position where these two forces are balanced. The ring upon the tube is then slipped to where the index rests.

The next, and each successive observation, is to be made in precisely the same way, *i.e.* by placing the bulb in newly drawn water from the same well or spring,—and as the index is above or below the ring a diminution or increase in the weight of the atmosphere is indicated; showing the approach of storm or the reverse, just as in all other barometers.

In summer or in heated positions when the instrument is not in use the index may ascend to the top of the tube, and rest against the silk covering; and during winter or in cold positions it will rest upon the lower covering. The object of these coverings is to prevent the escape of the mercury, while at the same time giving a free passage to the air of the atmosphere.

This barometer it will be seen is quite portable, and very cheap.

The *second* of these improvements is quite ingenious and philosophical, being both barometer and thermometer combined in the same instrument. Its construction and operation are precisely similar to the "Farmer's Barometer," except that the tube is longer, say 12 to 18 inches, and that it has a scale attached to it. This scale is graduated for temperature on one side of the instrument, from top to bottom; and on the other side are marked the inches and their parts, for the barometer.

As a *barometer* this instrument acts precisely as the former, already described, and therefore needs no further explanation, the only requisite as with the other, being to bring it to one and the same temperature at each observation.

And now to illustrate its action as a *thermometer*, let us suppose that the uniformity of temperature is obtained not by spring water, but by placing the bulb in the mouth, thus giving it the temperature of the human body 98°.—Now it will be understood that the temperature of the air will be told approximately at all times by the index, when the bulb is not in the mouth. The index, however, is being acted upon by the atmosphere from without, and consequently this approximate temperature must be corrected by adding or subtracting according as the index is above or below the standard temperature, (98° in the case supposed) when an observation is made. Suppose, for instance, that the index is at 50° when the instrument is in the air, and rises to 98° when the observation is made, then the temperature of the air is just 50°; but if the instrument had gone 10° higher, that 10° is owing to a lightness of the atmosphere and must be added to the 50° to give the true temperature, viz., 60°; but on the other hand if the index falls 10° short of the standard, it is owing to an increase in the pressure of the atmosphere to just that extent, and consequently the 10° must be subtracted and the true temperature is 40°.

The standard of this instrument is 98° and 30 inches.

If this instrument be properly made, and a true uniformity of heat applied, no thermometer can be more accurate.

The third and last improvement, I have the pleasure of communicating is the lengthening of the scale of the common mercurial barometer to an indefinite extent. Thus the *tube* of the common parlor barometer, instead of the wheel and its counterweight in the cistern, is furnished with a thermometer tube three feet long, open at top and bottom, and standing side by side with it, wherein the rise and fall of the mercury will be seen on a very largely increased scale. This additional tube has its lower end widened to, say, twelve times the size of its bore, and is partly filled with colored alcohol. This is set into the mercury in the open cistern. The specific gravity of the alcohol is so much less than that of the mercury, that a sufficient column of alcohol will be easily sustained in this additional tube, to be acted on by the atmospheric pressure. The lower end being enlarged in the ratio of twelve to one, it follows that a rise or fall of one inch of the mercury in the cistern will necessitate a rise or fall of twelve inches in the alcohol in the tube. Consequently a range of three inches of the mercury would be indicated by a range of three feet of the alcohol.

To prevent the evaporation of the alcohol in this constant rise and fall, the inventor places one of his small drops of mercury over the alcohol with about an inch of air between. This drop of mercury rises and falls with the alcohol. The top of this tube is covered with a piece of silk to prevent the entrance of dust, &c.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, June 16, 1864.

William Sellers, President, in the chair.

Fairman Rogers, Vice President, } present.
John F. Frazer, Treasurer, }

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

Donations to the Library were received from the Institute of Actuaries, and the Society of Arts, London; la Société d'Encouragement pour l'Indrustrie Nationale, Paris, France; Hon. William D. Kelley, U. S. Congress, Washington, D. C., M. W. Baldwin & Co., and Prof. John F. Frazer, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for May was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (5) were proposed, and the candidates proposed at the last meeting (13) were duly elected.

The By-Laws of the Institute as reported by the Committee and amended at the adjourned meeting held on the 9th instant, were again taken up for consideration, when they were further amended, and finally adopted.

Mr. P. E. Chase illustrated by a diagram the combined action of rotation and lunar attraction, which led him to believe that there should be four atmospheric spring tides, and four neap tides, in each lunar month, and stated that his theory was confirmed by the barometric observations at St. Helena, and in other places. He thought it would also be found that the same influences produce secondary spring and neap tides in the ocean, as well as in the air.

BIBLIOGRPHICAL NOTICES.

Practical Treatise on Limes, Hydraulic Cements, and Mortars. By Q. A. Gilmore, A. M., New York: Van Nostrand, 1863. 8vo. pp. 333.

This is one of the series of papers on Practical Engineering, issuing from the United States Engineer Department, and had we noticed the work when it was first published, perhaps it might have been necessary to say who the author was. Since then, however, he has so distinguished himself in his profession, that any notice of him is unnecessary. This work contains a record of certain experiments and researches made under the authority of the Engineer Bureau of the War Department from 1858 to 1861, upon the various hydraulic cements of the United States, and the materials for their manufacture. The experiments were carefully made and are well reported and compiled. It is strange that, in a matter so interesting to engineers, and apparently presenting no very great difficulties, except such as patience and science could overcome, so little should be known; but it seems certain that our knowledge of what is necessary to constitute a good hydraulic cement is yet almost at a minimum. What is known, and the means of knowing more, will be found in this work, which is presented with the usual excellence of execution of the publisher.

Elements of Chemistry. Theoretical and Practical. By William Allen Miller, M.D., LL.D. *Chemical Physics.* Part II. *Electricity and Magnetism.* From the 3d London Edition. New York: John Wiley. 8vo. pp. 191.

We do not pretend to know what Chemical Physics are or ought to be. All physical knowledge is important to chemists, and chemistry is a branch of physics; but however this may be, and whatever may have been the intention of the work, it is a very excellent practical treatise on electricity and magnetism, full and accurate and lucid in its expositions, and well illustrated by tables and wood-cuts.

Ewbank's Hydraulics.

The 15th edition of this interesting work is about to be issued by the publishers in New York. No further commendation of the book is necessary than this simple announcement that it has so rapidly reached a 15th edition, and that without receiving or requiring any of the artificial means of distribution so frequently used.

A Comparison of some of the Meteorological Phenomena of JUNE, 1864, *with those of* JUNE, 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	June, 1864.	June, 1863.	June, for 13 years.
Thermometer—Highest—degree, .	96·00°	91·50°	98·00°
" " date, .	26th.	15th.	29th, 1856.
" Warmest day—Mean,	89·67	80·33	90·50
" " " date,	26th.	15th.	30th, 1856.
" Lowest—degree, .	51·00	52·00	42·00
" " date, .	12th.	8th.	5th, 1859.
" Coldest day—Mean,	59·33	60·83	55·00
" " " date, .	11th.	7th.	6th, 1861.
" Mean daily oscillation,	17·42	16·02	16·43
" " " range, .	5·69	5·18	4·86
" Means at 7 A.M., .	67·33	65·78	68·48
" " 2 P.M., .	78·23	74·75	78·51
" " 9 P.M., .	70·03	67·90	71·08
" " for the month,	71·86	69·48	72·69
Barometer—Highest—Inches, .	30·087 in.	29·994 in.	30·281 in.
" " date, . .	21st.	25th.	13th, 1852.
" Greatest mean daily press.	30·028	29·984	30·251
" " date, . .	21st.	25th.	13th, 1852.
" Lowest—Inches, .	29·296	29·321	29·182
" " date, . .	9th.	1st.	11th, 1857.
" Least mean daily press.,	29·358	29·377	29·262
" " date, . .	9th.	1st.	11th, 1857.
" Mean daily range, .	0·127	0·083	0·100
" Means at 7 A.M., .	29·812	29·743	29·808
" " 2 P.M., .	29·777	29·712	29·774
" " 9 P.M., .	29·805	29·756	29·789
" " for the month, .	29·798	29·737	29·790
Force of Vapor—Greatest—Inches,	0·850 in.	0·687 in.	1·059 in.
" " " date, .	25th.	12th.	30th, 1855.
" " Least—Inches, .	·221	·207	·162
" " " date, .	28th.	16th.	5th, 1859.
" " Means at 7 A.M., .	·454	·436	·506
" " " 2 P.M., .	·475	·433	·526
" " " 9 P.M., .	·503	·446	·542
" " " for the month,	·477	·438	·525
Relative Humidity—Greatest—per ct.,	89·0 per c.	94·0 per c.	100·0 per ct.
" " " date,	5th.	22d & 29th.	6th, 1856.
" " Least—per ct.,	24·0	22·0	22·0
" " " date, .	28th.	16th.	16th, 1863.
" " Means at 7 A.M.,	65·9	67·9	72·0
" " " 2 P.M.,	48·4	51·0	53·6
" " " 9 P.M.,	67·1	65·6	70·0
" " " for the month	60·4	61·5	65·2
Clouds—Number of clear days,* .	10·	5·	8·2
" " cloudy days, .	20·	25·	21·8
" Means of sky cov'd at 7 A.M.,	48·7 per ct.	69·7 per ct.	58·6 per ct.
" " " " 2 P.M.,	54·7	66·7	60·5
" " " " 9 P.M.,	46·0	46·3	44·6
" " " for the month,	49·8	60·9	54·5
Rain.—Amount, . .	2·246 in.	4·053 in.	4·355 in.
No. of days on which Rain fell,	7·	11·	11·6
Prevailing Winds—Times in 1000	S 73°30′W ·343	N 61°23′W ·077	S 76°56′W ·228

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* Sky one-third or less covered at the hours of observation.

JOURNAL

OF

THE FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

AUGUST, 1864.

CIVIL ENGINEERING.

On the Organization of the Corps Imperial Des Ponts-et-Chaussées, in France. By GEORGE R. BURNELL, ESQ., C.E., F.G.S.

From the Journal of the Society of Arts, No. 591.

(Continued from page 12.)

THE ultimate consequence of the strict system of promotion that prevails in France is, that all that relates to the general direction of the public works of that country is obliged to be transacted through the body of the Ponts-et-Chaussées; and the civil engineers are there reduced to the rank of conductors of the industrial establishments, that are still allowed to be free from state control, although the civil engineers have proved themselves to be capable of directing the railways and other public works, with at least equal ability with their more fortunate rivals. The railways seemed, for a time, to be destined to form an exception to the general body of the works, for the late Mr. Locke and the English contractors were entrusted with the execution of the Paris and Havre and the Cherbourg lines: but it was found that the relations between the Minister of Public Works and the company were so numerous, and they comprehended so many questions which involved the rights of the Ponts-et-Chaussées to interfere with the working of the railways, that even upon these lines the government engineers have been employed. Indeed it is difficult to imagine any other course being long adopted in France; the whole service of that country is so desperately centralized that the intervention of the state is felt in all things. Thus, the body of the Ponts-et-Chaussées, being always upon the spot, is necessarily employed for making the surveys for the lines of railway, roads, and canals, that may be thought necessary; and the organization of that body provides it with ample means of carrying out the projects that it conceives. It may be that the nation gains in the unity of views that is thus produced, and that many cases of competing lines may thus be avoided; but this advantage seems to me dearly purchased, when it is only obtained by the annihilation of the private enterprise of the whole country, which is the inevitable result of the French system. It has been pretended that the estimates that are prepared by the French engineers are better and more trustworthy than those that are made by their English brethren, and that a contractor would be more inclined to risk his fortune on the French preliminary surveys and estimates; but if we set aside the greater facilities that the French engineers have for making their calculations for the works that they are employed upon, the superior correctness of their estimates in works that are at all out of the common run may be very much questioned; and certainly they design their works in a manner that English engineers would consider to be very extravagant owing to the traditions that prevail in the body of the Ponts-et-Chaussées, who pride themselves rather upon not considering the commercial view of the question in their constructions. It is, however, to be observed that much of the merit that is thus due to the engineers of the government is owing to the admirable system upon which their accounts are kept; and the merit of the "comptabilité" that forms, so much and so justly, the boast of the engineers is, after all, due to the conducteurs, whom the regulations of the body consign to perpetual inferiority. The engineers, in fact, content themselves with the duty of organizing the manner in which the estimates and the definite accounts are to be prepared; they leave all the details to be worked out by the conducteurs, who fill the position of the Soodras in the "hierarchy" in which their chiefs play the part of the Brahmins. There is much more truth than appears at first sight, in this comparison with the Indian social arrangement, for the classes that are thus formed in the French body politic are as strictly defined as those of India, and there is nearly as much difficulty in passing from one rank to another. The distinction between the ingénieurs des Ponts-et-Chaussées and the conducteurs is, however, a permanent and ineffaceable one, that will always be a standing reproach to the system that would prevent a man from rising by his own merits to the foremost ranks of his profession.

Yet, although I am myself fully aware of the inconveniences of the French system of organizing the public works of that country; though I know the great expense that the nation is put to on account of the want of practical knowledge, and from the deficient technical education of the engineers that the government is obliged to employ, from the fact that no man can there rise from the ranks, let his merits be ever so great, and from the mania that the system fosters for constructing monuments that should last for ever; yet I have seen such confusion and such waste occasioned by the system that prevails in England, especially in the appointment of government officials in similar matters that I should be almost inclined to prefer the French system of the Ponts-et-Chaussées to the ignorance and incompetence that prevail here in government offices. The engineers of the Ponts-et-Chaussées are thoroughly acquainted with the subjects they have to pronounce upon, and they all feel as though the reputation of their body were entrusted to their safe-keeping; whilst in England the nomination of the engineers of the government is entirely regulated by favor and caprice. Fortunately, the influence of the authorities that are appointed by the state is very small in England, for we still retain so much of the independence of habits and modes of thought that are the essential characteristics of local self-government, that the engineers named by the government can do little; but if the circumstances should change, and if the influence of the central government should be increased, it must become a serious question with us, whether the organization of the Ponts-et-Chaussées, that does not admit the nomination or the advancement of the favorites of the minister of the day, may not be the best that can be devised in the interest of the rate-payer. In a highly-centralized government, like that of France, the organization of the Ponts-et-Chaussées no doubt produces excellent results. It would be out of place here, as long as we retain our self-dependence and that love of true equality, that enables any man to rise according to his own merits; but it is the best system that has yet been thought of for securing the talent of the men and the independence of their judgment from the theories or the passions of the moment. There are several details in the organization of the service which might easily be altered (as, for instance, the fact that the engineers are only responsible for the acts that they may do in the discharge of their duty to the *Conseil d'Etat*, and the fact that the conducteurs cannot rise to the superior ranks of the profession), but, with these exceptions, there seems to be no reason why something like the organization of the Ponts-et-Chaussées should not answer very well with the gradual tendency to the development of centralization that is so manifestly gaining ground amongst ourselves. It would secure for the public at least the services of educated engineers, and put a stop to the curious nominations that we have sometimes witnessed in our public offices; added to which advantage would be the incidental one of raising the tone of the profession in matters connected with their own emoluments, which recent events have shown to be very low in England. Perhaps there is something that is opposed to the English notions of government in the existence of a body that is composed of officials, who are not in any way liable to the influence of the ministers of the day, either for their nomination or for their advancement, and there may be danger in thus creating a body that should have the control of the funds of the nation to the exclusion of the rest of their profession, and to the perpetual interference with the ingenuity of the great body of the public; but this is counter-balanced by the advantage of the absence of the influence of the minister. There is, of course, the disadvantage of having to do with a board of theoretical men, who are likely to insist upon the observance of the strict scientific conditions that they conceive to regulate their pursuit, as was the case with the Ingénieurs des Ponts-et-Chaussées in the matter of the rate of the inclines for railways, which they for a long time insisted should be made with an inclination of 1 in 200, when the English engineers had proved that they might be executed at 1 in 60 without inconvenience. This is a drawback that will always attend an organized body of such a nature; but there must be set against it the advantage that this body procures in controlling the action of the government in the details of the practical application of the science. The minister is, indeed, quite powerless in cases which strictly belong to the carrying out of works that may be ordered; the Ingénieurs des Ponts-et-Chaussées alone can regulate the manner in which they are to be executed. The fact that the Minister of Public Works is a political appointment, and therefore is subject to the caprice of the parliamentry government, throws a great power into the hands of the Ponts-et-Chaussées, who are always in possession of the influence which they may derive from their position as distributors of government patronage, and as executing the works that the state may, through their advice, undertake; and this they have shown that they know how to use for securing their independence. It is desirable here to mention that the course adopted in the execution of public works in France is for the Minister of Public Works to submit to the legislature a project of law for their establishment, if they are of a nature to require a concession from the state, or to give rise to a large advance of public funds, or if they should require the application of the law of expropriation; in other cases, they may be the subject of simple ordinances, as in the case of municipal or departmental works. The projects for all of these undertakings must be accompanied by drawings, specifications, and estimates, which must be approved by the conseil that is specifically concerned in their execution, and they give rise to the production of a mass of papers and reports that are produced for them, and which are themselves perfectly bewildering. The worst of this method of entrusting the public works to the care of the state is, that there is no security that they shall be finished if once begun, provided the state be in want of funds, or it should have been found advisable to appropriate them to other uses. This last contingency has, it may be observed, already happened in several remarkable cases. For instance, the breakwater of Cherbourg, which cost the total sum of £2,400,000, had been mounted by the simple interest of the money during the time that it was in hand, to the sum of £8,400,000. The cost of the canals had been just tripled by the interest that had accumulated upon the sums invested in them before they were inaugurated throughout their length. Besides this there is always the danger and difficulty of the works being executed in an official manner, that gives rise to the inconvenience of reports and reference to the various offices of the state, in case there should be any reason to change any of the details of the system that may have been agreed upon at the first conception of the scheme. All the questions must be decided in the

Conseil des Ponts-et-Chaussées of Paris, and, setting aside for the moment the absence of local control that must thence issue, there must be a tendency in that body to impose there own style of work and their own modes of thought. In the cases of the works that are conceded to large public companies it might be supposed that the administrations of the latter would have sufficient power and influence to reduce the interference of the body of the Ponts-et-Chaussées to their legitimate bounds, but in all the concessions hitherto given there is left so wide a margin for the action of this body that they are practically all-powerful. The works are to be executed to their satisfaction, and they have a control over the workmen, the materials employed, and the manner in which the work is executed, so that they can stop the companies at any stage of their progress. Hitherto the force of public opinion has kept the engineers within the bounds of what may be considered their duty. It remains to be seen what will be the effect of this system when it is applied to a country fully accustomed to centralization, and debarred from the control of a free press, such as France is at the present day. The tendency of what is now passing in that country is to introduce the control of the engineers independent of the minsters of the day; to establish the system of the monopoly of classes that is perpetuated by the necessity for the pupils passing through the Ecole Polytechnique; and by the exclusion of the conducteurs and the civil engineers, to concentrate the money and the patronage of the state entirely in the hands of the engineers of the Ponts-de-Chaussées. I think this must eventually prove to be a wrong policy, though it certainly tends to raise the character and position of the engineers; and that France will, sooner or later, find herself, in this respect, in the trammels of a system of castes that will effectually destroy all originality of thought and action.

Discussion.—Mr. Haywood (Engineer to the City Sewer Commission) said he did not gather from the paper that Mr. Burnell held any conclusive opinion as to whether the system in France was better than that in England, or the reverse. With a great deal Mr. Burnell had said he fully concurred, but the root of the whole question of the organization in this country of a corps corresponding to that of the Ponts-et-Chaussées lay in the political condition and habits of the people. Whether it would be desirable or not, there was little probability of its being done in our time. He must confess that being tolerably well acquainted with France and her institutions, he never went there without regretting that some system could not be established in this country by which more unity of action in engineering matters could be obtained especially with regard to the metropolis; but he could not see any intermediate condition between the present local-government system and the institution of a corps somewhat similar to that of the Ponts-et-Chaussées. If that corps was tested by the grandeur of its works, it was far in advance of the English; on the other hand, if the results were tested by the activity of commercial enterprise; then the French system was far behind ours. They had then to choose between the two. With regard to the works of the engineers in France, his conclusion was that their aqueducts, docks, high roads, and canals, were laid out better than our own, and also that the workmanship in them was superior to ours. With regard to the originality of their works he did not feel he could give an opinion. As to their scientific investigations he was inclined to place a higher value upon them than Mr. Burnell had done. Undoubtedly the first series of engineering publications he was acquainted with was the *Annales des Ponts-et-Chaussées.* He knew nothing in this country approaching them, and they formed a scientific literature of which any nation might be proud. There was one thing he could fully endorse,—that was the very high tone of honor amongst the engineers themselves. Their official pay was exceedingly small, but he believed a body of men with a higher professional tone did not exist. They did not aim at acquiring large fortunes, inasmuch as scientific men generally got in France that which they could only obtain here by becoming wealthy—they got high honor and social position. He happened to be at the New Year's levée of the Emperor, and he then inquired what was the precedence of the Corps des Ponts-et-Chaussées on such an occasion. He was told that the engineers took rank before colonels in the army, which was an evidence that their standing was fully acknowledged. But that was not the case in this country. There were brilliant exceptions, such as Faraday, and some others who, although they never sought for wealth, were still so distinguished that they could not be overlooked; but the great mass of professional men in this country were generally measured by the depth of their pockets. That was why they struggled to make money as quickly as possible, because money was the passport into society. The engineers of the Ponts-et-Chaussées had an entirely different tone of feeling. He knew many of them personally, and could speak of them in the highest terms. There was one remark in the paper about the architects which struck him. He understood Mr. Burnell to state that they were responsible for what is called the well-designing of their work for a period of thirty years. The period was a long one, but he thought it would be a wholesome thing if some such rule were acknowledged in this country. It was true, a man was responsible for what he did, because if he did not perform his duty, remedy might be had against him at law; but most persons would rather bear the loss than have recourse to the remedy. Then, again, the French system had its disadvantages. All those who practised as engineers in that country must have gone through the Ecole Polytechnique, and thus have been in some measure educated by the state, and so became naturally subject to state control. After all he felt that he could not say with certainty which system he preferred, there were so many elements involved in the question; but he would say he never went to France without coming back bitterly regretting that our present system did not, with our huge wealth and great commercial enterprise, produce results more worthy of the nation. If a few remarks on a subject, now becoming one of great interest in this country, were not out of place, he would say a few words as to the system of sub-ways formed in Paris, which would serve as an illustration of the great care with which such works were carried out in France. His remarks would apply to Paris, Lyons, and other large towns. In the whole of the internal arrangements of those towns the French were beating us hollow: they were building better houses, forming better thoroughfares, putting up better lamp-posts, giving more light, and in all arrangements which affected the health of the community they were considerably in advance of this country. With regard to the main sewers of Paris, they were built in a style of magnificence which might be expected from a body who were not limited as to expense. He was one of those who thought the public works of large cities ought to be monumental, and not merely for the present generation. Surely it was an unworthy feeling that it was sufficient to carry out works which would last our own time, and to leave posterity to take care of itself. That was part of the money-greed of the age; and in that respect our public works contrasted unfavorably with those of Paris. The large intercepting sewers of the French capital were made with footways on each side. Those along the Rue Rivoli and the Boulevart-Sebastopol were so large that a full-sized man could walk on the footway on either side with his hat on, so ample were the dimensions of these sewers. They were kept in a state of cleanliness that was surprising. The entrances were made highly ornamental, which he thought superfluous; the steps which led down to them were wide enough for two persons to descend side by side. A railway ran along the edge of each of the footways of the large sewers, and below that was the invert forming the sewer proper. An exceedingly well-devised apparatus traveled up and down this railway, and mechanical scrapers were used by which the bottom of the sewer was constantly cleansed. The whole of the gas and water pipes were covered with a black varnish or bituminous compound; it was true there were leakages, but very few. The smaller branch sewers were most of them six feet in height, and on each side of them might be seen porcelain figures marking the house drains, with a small piece of white enamel about an inch square, which indicated that the proprietor of a particular house paid three francs annually for keeping his private drain clear. Every thing was carried out on the same scale. When they contrasted this with the way things were done in London, without entering into the political question, with which he had nothing to do, but looking simply at the material results as they were presented in the capital and large towns of France, it made us ashamed on looking at our own metropolis; and when one knew as much about the expenditure in London, as he did, as compared with the wealth, he must come to the conclusion that there was no town in France in which the inhabitants relatively to their means spent so little in keeping their city in decent and proper order as they did in London.

Mr. E. C. Tufnell remarked that the direction of the observations just made was altogether to depreciate English engineers below the French. He did not agree with that position, because there were many facts which went in contradiction of it. When the French desired to introduce any new engineering practice they almost invariably came to this country for the assistance of engineers. When railways were first commenced in France they sent for Locke and other English engineers to carry them out. We were the first country to introduce suspension-bridges. The French sent a deputation to this country from the Ecole Polytechnique to inspect what we had done; they returned to France, issued an elaborate report, stating that our suspension-bridges were erected upon wrong principles, and devising (as they thought) better ones. They erected a suspension-bridge in Paris on their principles, but immediately upon its completion it tumbled down. Then, again, with regard to the Suez Canal, notwithstanding the predominance of French influence in Egypt, the Pacha sent to this country for engineering advice with regard to that project, and Mr. Hawkshaw went out to that country to make a professional report upon that great undertaking. Looking at these facts he could not at all reconcile them with the views expressed by Mr. Haywood.

Mr. Frederic Lawrence did not feel the same difficulty that Mr. Haywood felt, as to which system he preferred. As an Englishman he approved of a system which allowed talent to develop itself—by itself—rather than a centralized system which tended to cramp the energies and fetter the originality of those employed; and he thought if they compared the engineering works of this country with those of France, looking at the works themselves, they would see at once that our system was by far the best. Could the French engineers point to a Britannia Bridge? or if they had anything resembling it did they not copy it from us? Was not every large engineering work in France a copy or modification of something previously done in England? He was surprised to hear Mr. Haywood speak so approvingly of the sewers of Paris. It might be that the sewer along the Rue Rivoli was better than anything we have in London, but why was it better? Because the French engineers came over here and saw what we had done: they adopted what we had done well, and improved where they felt they could improve. It was, however, only recently that sewers had been introduced into Paris, and if that system were carried out all over the capital—as was stated regardless of expense—Paris might eventually be a better drained city than we could boast of. With regard to some of the details, he did not think Mr. Haywood, with whatever staff he could command, would care to have put upon him the task of fixing up the tablets he had spoken of, in the city sewers, indicating the numbers of the drains, and the sum paid by each private individual to have his drain kept clean. He had no hesitation in saying that, in his opinion, however admirable an institution the Corps of Ponts-et-Chaussées of France was, it bore no comparison with the engineers of England; because, when he looked round, he saw the names of engineers who had carried out works which the Corps of Ponts-et-Chaussées dared not have attempted. He therefore differed from Mr. Haywood, and had no hesitation in pronouncing the English engineers the first in the world; for, when any country was in difficulty, England was applied to for that valuable engineering counsel which could be obtained from no other country.

Mr. Haywood explained that his remarks had been misunderstood. He had dealt only with the broad features of the paper, and he had made no comparison between English and French engineers, as regarded their professional skill.

Mr. Lawrence understood Mr. Haywood to refer to the question of the two systems, and they must judge from the results whether one was better than the other.

Mr. Burnell said he had carefully guarded himself in what he had said against drawing comparisons between English and French engineers. Upon the question the relative merits of the two classes is was a singular fact that the French engineers had almost invariably consulted the English, and all the inventions that had altered the face of engineering science had arisen either in England or America. With regard to railways it was well known they originated in England, and the same thing occurred with reference to suspension-bridges. The merit of that form of bridge they knew belonged to Telford. Steamboats, the electric telegraph, and many other inventions, which had tended to advance civilization, were the results of the free-trade principles which prevailed in England, and of which he was a cordial supporter. The object of the paper was to call attention to the organization of the corps of the Ponts-et-Chaussées, which he thought, for carrying out works necessarily in the hands of the government, was superior to our system of appointing Government officers.

The Chairman, in proposing a vote of thanks to Mr. Burnell, said he thought they must all, to a great extent, participate in the feeling expressed by Mr. Haywood, on comparing the style of the public works in Paris with those of London; and with regard to the sewers, if anything was calculated to mitigate that feeling, it was, as far as he was personally concerned, in the visit he paid to the great brick tank at Barking, covering an extent of eleven acres, which for strength and excellence of execution could hardly be surpassed, and which showed what could be done in England in the way of first-rate brick-work. He was afraid they could hardly expect to settle the question this evening as to the comparative merits of the two plans, and he thought they must be content with supposing that each nation had settled down instinctively into the particular system which best suited its own peculiar constitution. He was sure they would pass a cordial vote of thanks to Mr. Burnell for his able paper.

The vote of thanks was then passed.

Spoked and Disk Wheels.

From the Lond. Practical Mechanic's Journal, April and May, 1861.

We have much pleasure in presenting to our readers the following interesting and important conclusions, which have been very elaborately wrought out by the celebrated French philosopher, M. Ad. Martin.

The large development of the railway system has caused the manufacture of the wheels of rolling stock to become one of immense importance.

The number of axles belonging to carriages and vans in France, at the 1st of January, 1862, was about 85,000, of which 3000 having disk wheels attached to them, represented a value of more than fifty millions of francs. The just combination of the several pieces of which they are composed, is one of the first elements of their security. The wearing out of the tyres is another most important consideration. It becomes thus a matter of great commercial interest that the wheels should be constructed after an improved type.

It is thought that all the requirements will be represented in the following terms:—

1st. To realize the greatest solidity amongst all the pieces.

2d. To offer as little resistance as possible to the action of the air.

3d. To be of light weight, and to offer as much resistance as possible to the binding energy of the tyre.

4th. To lift as little dust as possible when running.

Lastly. To produce at a minimum price an article of maximum durability.

Spoked wheels, which are employed for locomotive engines and rolling stock generally, are more costly, heavier, and offer a greater resistance, than those having plain disks, therefore they should be replaced by the latter. Another motive for employing these disk wheels results from their inferiority to extend a fire, should it occur in the carriages when traveling, and which spoked wheels to a large extent provoke.

The wheels of carriages and wagons being usually identical, there is no necessity to make a distinction between them. Their diameter outside the tyre being generally about 0·9144 metres to 1·05 metres, the first of which corresponds to about 3 English feet, and the second to 3 feet 7 inches nearly. It would therefore be of little benefit to go beyond this diameter, for if, on the one hand, the friction of the journal is slightly diminished, the weight of the wheel is increased, as well as the rolling friction, which, increasing as the diameter, destroys the advantage thus obtained.

The resistance that the atmosphere presents to the rolling action of wheels, is entirely independent of their diameter for a given rate of lineal travel, because in any case the circumferential velocity is equal, the number of revolutions corresponding with the diameter, accordingly as it is increased or diminished.

The general construction of spoked wheels being pretty well known, it is scarcely necessary to describe them in detail. They are usually made with straight spokes, the outer extremity of which are forged of a T form, these ends are then welded together in pairs, the manner in which the joint is made being one of the most important elements in the construction of spoked wheels; these are made secure at the centre, by a boss of iron cast over and between them. When the cast iron boss has become cooled, the outer rim formed by the T-shaped extremities is thoroughly welded, and made circular. There are many other ways of manufacturing spoked wheels for railway carriages and vans, and, indeed, they are now sometimes made entirely of wrought iron, in a manner similar to that introduced by Mr. Gooch, on the Great Western Railway, for the wheels of locomotives.

Wheels with plain disks are much lighter and encounter much less resistance with the atmosphere during their revolution, they do not lift the dust, are not easily disturbed, and hold equally well upon the axles and in the tyres. Any person conversant with this subject, will thoroughly grant that a wheel composed of one piece of malleable or cast iron, in the form of disk, is much more solid and secure than one made up of an assemblage of pieces. It is, however, contended by some, that by the use of disk wheels, the tyres would soon become flattened, loose upon the wheels, and at the same time the wheels become loose on their axles. These prophesies, however, have not been realized in practice—they are contradicted daily, and, indeed, it appears that disk wheels hold better in their tyres than spoked ones have ever done. In either case, the chief element of secure fastening between the tyre and the wheel consists in the amount of strain produced by the tyre on the wheel, this adhesion being also increased by the introduction between them at intervals of bolts or rivets; in either, the bolts or rivets, their action is identical. It remains, therefore, to be seen what the actual value of this binding action amounts to, in shrinking the tyre over the wheel, the contraction of the tyre being usually brought about by first heating it, then passing it over the wheel. When thus placed in position, both are immersed in a cistern of cold water, where they are allowed to remain until the tyre is sufficiently cooled. It should also be stated that, prior to the tyres being heated, their internal diameter as either rolled or bored, is to be slightly less than the exterior diameter, of the wheel, in order that when expanded by heat, and again contracted by cold, they hold the wheel very firmly.

Let $\text{P} =$ total pressure of the tyre upon the wheel.
$p =$ pressure per unit of surface.
$l =$ width of the tyre.
$r =$ radius of the felly or rim of the wheel inside the tyre.
$\text{R} =$ the co-efficient of elasticity of the iron, which, in the present case, is taken $=$ (12 kil.) for each square metre.
$\text{S} =$ the section of the tyre.

The total pressure of the tyre upon the wheel, and reciprocally, would be

$$2\pi rlp = \text{P} \quad . \quad . \qquad (1)$$

In determining the tendency to produce diametral rupture, it was found that

$$p2rl = 2\text{SR} \quad . \quad . \qquad (2)$$

In practice, $\text{S} = 140$ mill. $\times$ 55 mill. $= 7700$ mill. *en tenant compte du boudin*, when it would be—

$$pl2r = 2 \times 7700 \times 12 \text{ kil.} = 184800 \text{ kil.} \quad . \qquad (3)$$

$$r = \frac{184800}{l2r} \quad . \quad . \qquad (4)$$

making $2r = \cdot 94$ m. it would be

$$r = \frac{184800}{910 \times 140} = 1{\cdot}45 \text{ k.} \quad . \qquad (5)$$

The pressure of the tyre on the wheel for each square millimetre of its surface

$$\text{P} = 2\pi rlp = 580{\cdot}052 \text{ kil.} \qquad . \qquad (6)$$

The expansion or stretching of tyre necessary to produce this pressure of 580·052 kilog. per square millimetre would be

$$\text{A} = \frac{\text{P}}{\text{CS}} \qquad . \qquad . \qquad (7)$$

In which

A = the elongation of the tyre has in metres, for every metre in length;
P = resistance of the wheel tending to stretch the tyre in killogrammes;
S = transverse section of the tyre in square millimetres;
C = co-efficient of the modulus of elasticity;
Accrding to Poncelet, if P = 12 killog.;
C = to 20,000 kil.;

which would give

$$\text{A} = \frac{140 \text{ mill.} \times 55 \text{ mill.} \times 42 \text{ kil.}}{20{,}000 \times 7700.} = 0{\cdot}00060 \text{ m.} \qquad (8)$$

This expansion would then be less than one millimetre for every metre in length of the tyre, and would not produce a pressure of 580052 kil.

It follows from the preceding investigations, that if the wheel itself were positively inflexible, the smallest diminution or increase of the diameter of the wheel under or over that of the interior of the tyre, would either convey a diminished or increased pressure upon the interior surface of the tyre, the latter of which represents the energy tending to burst it.

The question arises, Are disk wheels elastic? It is absolutely necessary to obtain exact knowledge on this point.

In spoked wheels, the total pressure of the tyre spreads itself over nine double spokes, that is eighteen single ones, therefore the energy of compression of the tyre that each one of these supports, will be expressed by

$$\frac{580052 \text{ kil.}}{18} = 32225 \text{ kilog.} \qquad . \qquad . \qquad (9)$$

the dimensions of spokes being generally—

	m.
Length, . . .	0·290
Breadth, . . .	0·073
Thickness, . . .	0·045

According to M. Morin, the weight which each square centimetre of the section of the spoke would support, would be relatively to the preceding conditions = about 600 killogrammes, giving therefore

$$7\text{c}.5 \times 1\text{c}.5 \times 600 \text{ kilog.} = 6{\cdot}750 \text{ kilog.} \qquad . \qquad (9)$$

of contraction, so that the tyres could exercise upon each spoke a pressure of 32225 kilog., but exercising in reality only about 6750 kilog., that is to say, a fifth or thereabouts, which would reduce the entire energy exercised by the tyre upon the felly to 18 × 6·750 kilog. = 121,-

500 kilog., but it is supposed that the pressure which the tyre can effect, in the conditions about to be shown, that the centre of the wheel can support it, and if, as is seen, that the centre of the wheel can support a strain five times less, it will follow that the iron of the tyre will endure a tension which would correspond to the resistance offered by the wheel itself, and also it will be diminished in a proportion corresponding to the yielding or bending of the spokes at the time of putting on the tyre. It should be admitted, that in wheels the spokes of which are straight, and of a thickness of 15 millimetres, the tyre could not exercise an energy beyond 2·40 k. for each square millimetre of its own section, and that this would always be either above or below this amount in a proportion corresponding to the stiffness or flexibility of the spokes. It is then enough to state, that spoked wheels do not offer to the binding energy of the tyre a resistance sufficient to utilize the entire holding action it affords.

In the conditions of maximum pressure, the power that it would be necessary to exert to cause the tyre to slip on the wheel, would be 36,450 kilogs., taking 3 as the co-efficient of friction.

It becomes then advantageous to increase this binding energy by the insertion of five or six bolts or rivets, which for this purpose are usually of 0m. 035 diameter, and possessing a capability to resist shearing of about 50,000 kilogs.

The tyre influencing and exercising ordinarily, at the time of being put on the wheel, a pressure far greater than that which it (the wheel) can support, since with only a slight difference of the diameter there results an error which, by precaution, is always very great, in consequence of the bending of all, or of one part of the spokes, thereby producing an eccentricity of the journal with the circumference of the tyre.

Disk wheels on the contrary, are never out of shape, and present, at each point of the circumference, an equal resistance, which is greater than that of the wheels with spokes, and they have tyres and journals always concentric. The author has sought to determine the elasticity of a disk wheel by calculation; but precedents and formulæ are wanted to enable him to apply himself to the case, it has, however, been managed by proceeding experimentally, and much assistance has been obtained by the facilities and good feeling that he has met with at the works of the Orleans Railway Company.

From the following it will be seen how powerful the contraction of the tyre is, and how inferior the resistance of the centre of the wheel is to it; guided by these calculations, it has not been hesitated to put on the tyre of a disk wheel, *avec un ecart de diamètre* of 7 millimetres.

The tyre was 55 millimetres thick, with 127 wide. The centre of the disk wheel, furnished *par la Societe Providence*, had the following dimensions—

Internal diameter, . . .	0·720 m.
Breadth of the felly, . . .	0·082
Thickness of the rim of the felly, . .	0·013
Thickness of the side, near the felly, . .	0·018
Weight of the disk, . . .	130k.
Weight of the tyre, . . .	192k.

The wheel as well as the tyre was new; this last, made of iron and steel, had been furnished by M. Verdier.

In order to see whether or no the tyre could contract the disk wheel, three gauges were prepared, the first with points at a distance apart of 0m.65, the second 0m.40, and the third at 0m.30.

These measures, applied to the disk, equidistant from the centre on a diametral line, have been, as well as the diameters, set off with all possible care, and verified by many repetitions.

It was found on taking the tyre from the furnace the first time, that the dilatation was not sufficient; it was then put back in the furnace and brought to a red heat, then put over the wheel, where it was left until its temperature was reduced to that at which the tyres are tempered. Taken out of the water it was found sound, and the following statements are made concerning the body of the wheel:—

The diameter of the wheel had diminished 5 millimetres.
Two points, distant 0m.65, were drawn together 2·5 millimetres.
Those distant 0m.40, were drawn together 1·5 millimetres.
Those distant 0m.35, were drawn together ¾ millimetre.

These show, in the most complete manner, what it was wished to establish—the elasticity of the disk wheels.

As regards their resistance, it can be said that it is proportionate to the power of contraction of the tyre : : 2 : 5 since the tyre is enlarged 2 millimetres, whilst the wheel has diminished 5 millimetres; it is equally true that the tyre having stretched to its limit of elasticity, had attained a permanent lengthening which ought to be deducted from its total, to have the force *vive* of the tyre left free.

In order that a tyre, put on a disk wheel, might cease to be affected by it, it is necessary that it be lengthened; but the resistance of the iron being sensibly the same towards the traction as towards the crushing, the weight of a wagon has no bearing with the force that it would be necessary to exercise to produce that lengthening.

(To be Continued.)

On the Testing of Chain Cables. By Frederick Arthur Paget, Esq., C.E.

From the Journal of the Society of Arts, No. 598.

It is, no doubt, generally known that a select committee of the House of Commons is now considering a bill for the compulsory testing of the chain cables and achors of merchant vessels. This may be said to lend a passing interest to a question which, however, needs no chance help in calling for our attention.

Without entering into lengthy statistics, or calculating the number of times that the total length of all the chain cables in actual use would measure round the world, we should be scarcely mistaken in the supposition that in different parts of the globe there are, at this very moment, many hundreds of valuable lives, and thousands worth of property, in each case dependent upon a single link of the hundred fathoms that make up the length of an average chain cable; for there are situations in which a ship is often placed wherein the cable must be literally the thread of life of the vessel. To the seamen of the present age, the iron cable, though of comparatively recent introduction, is a common every-day thing. Those of the last generation could remember the time when only hempen cables were in use. The naval men of that time were thus led to look upon chain cables as the most precious gift ever made in modern times to the mariner—to repeat the words of the late distinguished Captain Basil Hall.

Now, although we have been testing chain cables according to certain Admiralty regulations established ever since 1831, although the naval administrations of France, Russia, and other countries have exactly copied these regulations, and although Lloyds' have adopted the Admiralty test—which is somewhat more than the so-called "merchant" test—it is a remarkable fact that a difference of opinion with regard to almost every point connected with the use and testing of chain cables still exists amongst engineers and other men of science. This differing of doctors is very strikingly shown by the Blue Book report from the 1860 Select Committee of the House of Commons on anchors and chain cables for the merchant service. One witness states that 50 per cent. of the loss of life by shipwrecks are due to bad cables and anchors; another that very few wrecks occur through bad anchors and cables. One objects to the Navy proof as being too high; another as too low. One witness considers that the cross-stay does not add to the strength of the link; another that the cross-stay is a great improvement. In the same way, directly contrary opinions were elicited from different witnesses with regard to the duration of cables under wear, their re-testing, re-annealing, and other points. A similar want of agreement on these matters exists in France; and it would thus appear that several interesting engineering questions, connected with the strength, the testing, and the re-testing of chain cables, offer a fair field for a practical examination.

According to the Admiralty regulations, an iron chain cable has to consist of eight lengths, each 12½ fathoms long, including one swivel in the middle of every other length, and one joining shackle to each length. Neglecting the swivels and shackles, each link may be described as a cylinder, the axis of which is wound into a shape approximating to that of an ellipse. The width over all, or across the minor axis, is made 3½ diameters (full) of the cylindrical bar. The length over all, or across the major axis of the supposed ellipse, is six diameters. The cast iron stud across the minor axis is made 0·6 of a diameter in the centre, and one diameter at each of its ends. This stud not merely acts as a cross-stay, but also preserves the freedom of the joints, or what may be termed the mechanical flexibility of the cable. The weights are of course exactly fixed in the government tenders. The weight of, for instance, a one inch link stay-pin must not exceed 3½ ounces, and the weight, fixed by contract, of a hundred fathoms of cable, in eight lengths, including 4 swivels and 8 joining shackles, must not be exceeded by more than 1-20th part. The experience of many centuries has determined the sizes of hempen cables for ships of a given tonnage; and, the sizes of the hempen cable being thus given, it is easy to substitute a chain cable of the required strength. Mr. J. R. Napier has proposed a formula, according to which one-eighth of the cube root of load displacement would give the diameter of the chain cable usually employed by steamers of the present form. In the Admiralty comparative table, showing the weights and strengths of stud chains and hempen cables, there is a noticeable relation between the girths of the hempen cables and the diameters of the iron employed in chain cables. The number of inches of the circumferences of the hempen cables pretty nearly expresses in lines, or twelfths of an inch, the diameters of the iron cables of equal breaking strength. The material of the links is No. 3 rolled bar, and very good cable bolts generally cost from £1 to £2 above common bars. According to experiments by Telford, Hodgkinson, Mr. Edwin Clark, and Mr. Kirkaldy, and also according to numerous experiments at Woolwich, we may safely take the ultimate breaking strength of cable bars at 24 tons to the square inch, and their limit of elasticity, under a tensile stress, at 12 tons to the square inch. These bars would stand a pressure up to deformation of 18 tons to the square inch, and 15 tons pressure at the elastic limit. The ultimate tensile strength of a round bar of this iron would thus be nearly 19 tons. According to the evidence of the leading man of the test house at Woolwich, in 1860, this ultimate statical breaking strength is only occasionally exceeded, when it rises up to about 20 tons for one-inch round bar, or 25·33 tons per square inch. He also stated that a great number of experiments, made at Woolwich, showed the greatest breaking strength of one inch chain cables to be only 28·31 tons. Contrary to the popular assumption that a stud link should be, in the direction of its length, twice the strength of a single bar, this result would show a loss in strength of 28·75 per cent. According to the comparative table published by the Admiralty, the one-inch bolts should stand 21 tons 8 cwts., and the stud link therefrom, 34 tons 5 cwts. It also appears to have been assumed (for it could scarcely have been proved by experiment) that the strength of the cable bolt, and of the link therefrom, both increase almost exactly in the ratio of the diameter of the bars. Thus, the breaking of two-inch bolts is given as 21 tons 8 cwts., $\times 4 = 85$ tons 12 cwts., to which two tons are added; the strength of chain therefrom as 34t. 5 cwt. $\times 4 = 137$ tons, and the proof as $18 \times 4 = 72$ tons. It is, however, well known to engineers that, as a rule, a two-inch bar is not practically four times as strong as a one-inch bar of even exactly the same make and by the same maker, and that the strength becomes less and less as the bulk still further increases. The proportions adopted by the Admiralty appear, however, to compensate for this loss, and there is very nearly the same average ratio of breaking strength to diameter in all chains from five-eighths to two inches. But, even according to the Admiralty tables, there is a remarkable amount of strength lost in forming the iron into the link. This loss of strength was well known to Sir Samuel Brown, the introducer of chain cables. He thus patented, in 1817, the straight link used in suspension bridges, and first applied it to the Brighton chain pier.

There are several reasons why a portion of the strength of the bar should be lost in forming it into a cable link. The principal causes are:—1st. The mechanical shape of the link; 2d. The crushing stress undergone by the insides of the crowns; 3d. The deterioration in strength of the iron through its being bent; 4th. The loss of strength at the welds.

In the first place, each link is, when the cable is pulled in the direction of its length, subjected to a transverse strain at each of its ends or crowns, and is somewhat in the condition of a curved beam loaded in the middle. An originally curved beam is, with regard to bending stress, in the same condition, at any cross-section at right angles to its neutral surface, as a straight beam under the same moment of flexure. The moment of flexure of one end of a common unstayed link can be expressed in inch-pounds by multiplying half the span, or half the distance in the clear, by the load in pounds. In the case of the stayed link, however, the moment of thrust of the cross-stay has to be subtracted from the moment of the bending force. The mechanically weakest part of any link is thus at the crowns.[*] Now, it is a curious fact, that all the writers on the strength of materials, from Professor Peter Barlow, Mr. Edwin Clark, and others, down to General Morin, in 1862,

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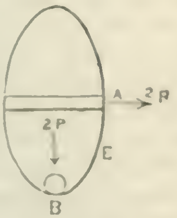
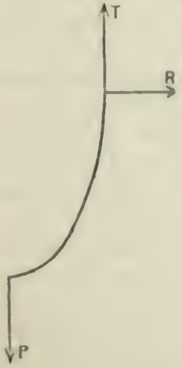
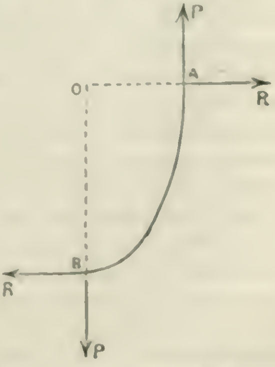
[*] For the sake of simplicity, let us suppose the cross-sectional area of the link as infinitely small compared with its major and minor axes, and suppose it provided with a cross-stay. Let 2P denote the whole pulling force; 2R the thrust of the stay; T the tension at A. For the equilibrium of the quarter link, B, E, A, we have the forces P, R, T, and the forces at B arising from the left-hand quarter at A. From symmetry this must be horizontal (in the figure), and we must therefore have:—Force at B = R, and T = P. The moment of the bending force at B is therefore not P × OA, but only P × OA, — R × OB. On the other hand, when the link is on the point of breaking by opening at B, the tension will not be equal to the ultimate tension throughout the section at B, but only at the lowest point, and when this has given way a little, the tension, previously supported, is thrown on a fibre higher up, which then gives way, and so on. Hence the strength is less than if the tension were throughout the section as great as possible.







give the strength of a link furnished with a cross-stay to be equal to that of the iron of which the link is made.

In a mathematical sense, the contact between the links is only at a point, because it is a case of two cylinders touching each other at right angles. Under a load, this point will spread out to a surface of an area given by the amount of the load and by the compressibility of the iron. This surface will then probably increase, in the case of a one-inch cable under a load of nine tons, up to more than half a square inch. And thus at the ends, the softer and more ductile the iron, the sooner will it be worn away in practice, and the progressive deterioration caused by this crushing action will also be furthered by the friction.

An attempt to account for the reduction in strength through the bending of the cylindrical bar has next to be made. Wrought iron is known to be a crystallized body, belonging to the cubic system. Now, Mr. Mallet, in his important work "On the Physical Conditions involved in the construction of Artillery," has shown that these crystals are not grouped amorphously (or without distinct arrangement); but that they always take a certain determinate position. He has developed the law that "iron, whether in the state of cast or of wrought iron, has the principal axes of its integrant crystals arranged in the lines of least pressure within the mass," while exposed to pressure and heat in progress of manufacture. The principal axes of the crystals in a rolled bar would thus be set in a direction coincident with the length of the bar, and, from the property of malleability possessed by these metallic crystals, they would further take, under the pressure of the rolls, or the impact of the forging hammer, the longitudinal extension known as the "fibre" of wrought iron. Mr. Edwin Clark found that bars cut longitudinally and transversely to the fibre of the same plate of an excellent quality of iron, gave with the fibre a strength of from 19·66 to 20·2, and across the fibre a strength of only 16·7 tons to 16·93 tons. The ultimate elongation also of the plate in the line of the fibre was double as great as transverse to it. A great number of experiments by Mr. Kirkaldy gave somewhat similar results. He found that the difference averaged from 21·7 to 2·1 per cent., the mean difference in the whole being 9·8 per cent. in favor of the direction of the fibre. The respective ultimate elongations were also in almost the same rates as those found by Mr. Clarke. It would thus appear from these experiments, and from a consideration of Mr. Mallet's law, that both the elastic range and breaking strength of wrought iron of any given quality are, to a certain amount, dependent on the direction of the crystalline axes in relation to the strain; and further, the elasticity would be at a maximum in the direction of the principal axes of the crystals, or "line of fibre." The crystals in a bar subjected in the direction of its longitudinal axis to a tensile or compressive stress, would thus be in the most favorable condition with regard to its ultimate breaking strength and its elastic limit. But when, say, a red-hot bar is being bent, the principal axis of its crystals would, according to the law of cross-bending strains, arrange themselves above and below a neutral axis in the direction of least pressure within the mass; the neutral axis would probably pass through the centre of gravity of the bar, the fibres on the concave side would be compressed, and those on the curved side would elongate in the ratio of their distances from the neutral axis. The hot iron itself would be, at any rate on the concave side, under a somewhat similar influence as when passing through the last pair of rolls, but the directions of least pressure, instead of being coincident with the length of the bar, would be at right angles to tangents to the neutral axis. Much of this is, of course, founded on several unproved assumptions, but it is at any rate evident that the molecular arrangement of the iron at the crown of the link is in the worst condition for resisting the tensile and compressive strains on each side of a neutral axis that make up the compound action of a transverse stress. The late Professor Daniel's process for unmasking the fracture and the arrangement of the fibre of wrought iron, by immersing the piece in dilute hydrochloric acid, would doubtless reveal a distortion of the crystals round a neutral axis.

It would thus appear that the crown of the link is its weakest part. This is, however, very far from being practically the case. Each link has of course to be welded up, and the weld is one of the sides, with a long scarf, in order to get a large welding surface. When we recollect that there are, in round numbers, 1800 links, and consequently 1800 welds, in a one-inch hundred fathom chain cable, and also that the efficiency of the cable depends on each individual link, the paramount importance of the welds is obvious. In nine cases out of ten, while in use and while being tested, the links are found to give way at the sides. Breakages would, *cæteris paribus*, have a tendency to occur at the welds with good iron but bad workmanship, and in the iron, and not in the weld, if good workmanship but bad iron were employed. The uncertainty of welds is in any case well known to practical men. Mr. Kirkaldy has made some eighteen experiments on the relative strengths of welded joints in wrought iron. Some of these welds were made by a chain maker. Only six of the specimens broke solid away from the weld, and in every case there was a loss of ultimate breaking strength averaged from 2·6 to 43 per cent., the mean being nearly 20 per cent. As with almost every thing else belonging to the subject of chain cables one of the witnesses before the Committee of 1860 raised the question whether the position at one of the sides was the best for the weld. Mr. Smale, of Woolwich, proposed to weld the link at the crown, as there would thus be more room for the smith, and any bad weld would be less hidden by the cross-stay. The crown is, however, as we have seen *ab initio*, the weakest part of the link. Besides, if a weld at the side gave way, the other half might catch and save the cable; at the same time, however, a sudden giving way at the weld would cause an instantaneous distortion and probable rupture of the opposite side, as the sudden "run" of the cable would act with an impulsive force. In fact, when iron cables were first introduced, the welds were made at the crown, but the plan had to be given up. It is clear enough that there are, *cæteris paribus*, three weak places in a link where any effects of stress would first show themselves,—the two crowns and the weld at the side.

We thus see what a powerful element of uncertainty is brought by the uncertainties of workmanship into such an apparently simple thing as a chain cable. When, however, we remember that the very best wrought iron of commerce is, to use the words of the well-known metallurgist Saint Claire Deville, but a metallic sponge, like platinum, the pores of which have been simply closed up by pressure or percussion; that, in one word, ordinary wrought iron has never as wrought iron, been fused, it will be seen that the uncertainties qualifying the material itself are still greater. Mr. Mallet thus found that while the original hammered slab of a very large forged mass had a breaking strength of 24 tons to the square inch, it fell progressively to 17 and 16 tons at the different places of the mass, down to even as low as 6½ tons in some parts. Unless this iron had been burnt its tenacity could doubtless have been restored, and if drawn into wire, its breaking weight might have been increased to perhaps 90 tons to the square inch—at least before annealing. An average of 188 experiments, made by Mr. Kirkaldy on rolled bars, gave a maximum breaking strength of 30¾ tons, and a minimum of nearly 20 tons to the square inch. These influences of the manufacture merely on the quality of wrought iron are almost independent of the chemical constitution of any individual bar. For instance, until it be proved to the contrary, there are many reasons for the general belief that the cold shortness of wrought iron is due to the presence of silicon and carbon; and its hot shortness to that of sulphur. A fractional per centage of copper also makes wrought iron hot short. In truth, there are probably no two bars or parts of a bar of an exactly similar chemical composition, or in an exactly similar state of molecular aggregation, and therefore of an exactly similar breaking strength or elastic limit. Even these are only a few of the elements of uncertainty in structural materials. But when we further take into account the varied strains of extension, compression, distortion, twisting, and bending, to which mechanical structures are more or less subject; that the work done by a gradually applied load is doubled if this load be applied suddenly; that the impulsive strain of a moving load is generally more or less intensified by vibration; and that the varied shapes and arrangements intended to receive these strains must be often as much fixed by financial as by scientific considerations, then the reason that the best engineering practice makes the ultimate strength of a wrought iron structure from four to six times the working load must be even popularly evident. But these factors of safety are not sufficient. The structure must be tested as searchingly, and as far as is consistent with safety—as far as is possible without injuring the material and its relation to the structure. In our case this limit is, in the main, given by the limit of elasticity of wrought iron under extension, as this limit is less for wrought iron than that of compression. It is also self-evident that the mode of testing adopted ought to approximate as nearly as practicable with the kind of stress the object is intended to undergo in practice. It is also evident that if circumstances allow us to exceed this limit, if, in fact, we can push the test as far as the breaking strength of a portion, or of an individual piece of the object, we shall obtain the safest amount of information about its qualities. In this way guns and plates are both tested to destruction. In order to test the probable performance of rails under a moving load, a certain number, taken from a lot, are broken by a falling weight, the distance between the supports and the height of fall being fixed by contract. The French test their railway carriage axles in a somewhat similar manner. There is no test so good as a blow for detecting a false weld. In Sweden they do not confine themselves to the usual gunpowder proof for gun barrels, but two or three sharp taps with a hammer are given along the breach, which have an immediate effect on a bad weld. All the whipple chains for the carriages of the Royal Swedish Artillery are tested by letting the loose end fall from a height double the length of the chain, after being attached to a weight. The anchors for the French imperial marine are tested by being dropped from a determined height for each size. The axles for the carriages of the Messageries Générales and the Imperial Artillery are tested by the impact of a falling weight. All the swords and sabres for the army are tested by striking the blades on a block of wood. When we advance from details to considerable structures, we are, of course, obliged to very carefully confine ourselves within the limits of elasticity. After loading a railway bridge with the greatest passive, or perhaps impulsive load that would ever come upon it in practice, the deflection, and the permanent set, if any, are both carefully noted. As a boiler is subjected in practice to a complex train of mechanical and chemical forces that are always striving to break their bounds, its ultimate strength is made from six to eight times the working stress, and it ought to be periodically tested to half its working pressure. Its extension under this pressure is sometimes—and should always be,—measured by the volume of water that is pumped in by pressure after the boiler has been filled; while the permanent set is determined by the difference between the volume pressed out by the contraction of the boiler when the pressure is withdrawn, and the volume of the water that remains in the boiler after the test—allowance being of course made for any slight leakages and sweating at the joints. In first-class locomotive works the deflexion and permanent set of the steel springs are always tested in an apparatus for the purpose. It may here be remarked that, although the designs of all the successful wrought iron structures ever built have been based on the assumption of a limit of elasticity, nevertheless the relation of the permanent set of wrought iron to its ultimate resistance under a given load, is still a subject of some discussion. We have, on the one hand, the testimony of Professor Eaton Hodgkinson, who says that "the maxim of loading bodies within the elastic limit has no foundation in nature;" and, on the other hand, some appear to believe that iron is even improved by breaking it under, at least, a tensile stress. Mr. Hodgkinson found that a rod, 10 feet long and of one square inch section, took a permanent set of 0·0005 of an inch under a static load of less than 1¾ tons. Mr. Edwin Clark obtained very similar results. Such a microscopic set, however, could be referred to the elongation caused by the heat generated by the internal friction of the particles, or to the probable fact that these bars were also new; and it is conceivable that they might have taken a slight permanent set, just as new rope takes a permanent set, without injury, when the strain is first applied. There are, indeed, very few forms of wrought iron in which its internal particles are not, *ab initio*, subject to some mutual strain. At any rate, these elongations were very slight, and increased uniformly up to tensions varying from about 10 to 15 or 16 tons on the square inch. Beyond these strains the bars elongated in an irregular manner until they at last broke. At the same time, as Dr. Rankine remarks, the demonstration by Mr. E. Hodgkinson that a set is produced by a strain much less than what would injure the specimen, renders the determination of the proof-strength a matter of some obscurity; but Dr. Rankine points out that the best test now known is the not producing an increasing set by the repeated application of a load. Some years ago, Mr. Loyd, of Woolwich, made certain experiments which have been cited as proving that a breaking strain does not injure iron, even when this strain is four times repeated; or rather, that after breaking a bar into, say, two pieces, the two pieces are thereby made stronger. In, for instance, experiment 2, the 1⅝ bar marked C was found to break with 33¾ tons, with a stretch of 9¼ inches in 54 inches; a piece of this bar then broke at 35¼ tons, with a stretch of only a quarter of an inch in 36 inches; another piece of the bar, 24 inches long, was broken at 37 tons, with a stretch of one inch; and at the fourth and last breakage was found to give way at 38¾ tons, but without any stretch at all. Results of a similar delusive kind, obtained by Professor Walter Johnson, were communicated by him to the United States government in 1845. He found that by heating a bar to a temperature of 400° Fahrenheit (or the temperature of steam at about 250 lbs. pressure), and stretching it permanently for about 6½ per cent. of its length, it, on being broken, gave an ultimate breaking strength about 20 per cent. higher than a portion of the bar that had not been heated and stretched. He therefore supposed that, to use his own words, "the value of useful purposes, added to a bar of iron by thermo-tension, when the increase of both length and tenacity is taken into account, may be safely set down at 26 per cent. of its original value. It sometimes exceeds 30 per cent. On a single cable, 100 fathoms long, made of iron 2½ inches in diameter, weighing about 15 tons, and attached to a line-of-battle ship, the gain, in true commercial value, would not probably fall short of 600 dollars." A machine was made by the American government, in order that the Professor might apply his principle of "thermo-tension" directly to chain cables, but as nothing else ever appears to have been heard about the matter, we have thus, as usual, lost another of the lessons always taught by scientific accounts of scientific failures. The pitch chains of the old engines of the *Great Britain* are stated, by Mr. Guppy, to have been stretched one-eighth of an inch while at a low red heat. This was, no doubt, an excellent method for testing the soundness of the work. Captain Blakely also stretches the hoops that are shrunk on his guns. This is done on a maundrel, and while the rings are at a red heat; but it is stated that only one-sixth of the breaking strain of the cold metal is applied. The action of the maundrel also probably re-arranges the crystalline aggregation which had been disturbed by bending the ring from a straight slab. The red heat of iron is only visible in daylight at a temperature of 1077 degrees Fahr., and the heat used by Professor Johnson was only from 400 to 500 degrees. But the "gain of length"—the permanent set, in fact—of from 5 to nearly 7 per cent., sufficiently shows that the bars had either been broken or were close upon fracture. His results, in fact, merely anticipated those of Mr. Loyd. The breaking strength of his bars was doubtless increased, but with a proportionate diminution, perhaps, indeed, a complete destruction, of their elasticity. They were rendered harder, for what is the hardness of a body but the resistance of its particles to any temporary re-adjustment. The longitudinal elongation was accompanied by a lateral contraction of the cross-sectional area that would reach its culminating point at the part where fracture happened to take place. Exactly the same argument, founded on similar experiments on cables themselves, was used before the 1860 Committee, in order to prove that cables are not injured by a breaking strain; but a mere statement of the progressive diminution of the elongations would have detected the fallacy.

(To be Continued.)

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, January 5, 1864.]

From the Lond. Mechanic's Magazine, Feb., 1864.

During the past month there have been examined 466 engines and 730 boilers. Of the latter, 11 have been examined internally, 75 thoroughly, and 645 externally, in addition to which 11 of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 13 (1 dangerous); corrosion, 29 (5 dangerous); safety-valves out of order, 2; water-gauges ditto, 11 (1 dangerous); pressure-gauges ditto, 24; feed-apparatus ditto, 2; blow-out apparatus ditto, 51; fusible plugs ditto, 8; furnaces out of shape, 2 (1 dangerous); over-pressure 6; deficiency of water, 1 (dangerous); blistered plates 1. Total 150 (9 dangerous). Boilers without glass water-gauges 7; without pressure-gauges, 7; without blow-out apparatus, 31; without back-pressure valves, 36.

Instances of serious as well as of dangerous corrosion, both internal and external, continue to be met with, which shows the importance of all the plates of a boiler being regularly submitted to careful examination.

Among other cases of corrosion which have been met with during the past month, the details of the following may be briefly given. In one instance, a boiler lately put under the charge of the Association, was found on the removal of a portion of the mid-feather, at the first examination, to be so eaten away by corrosion, that the merest touch made a hole through the plates, and several had to be renewed before the boiler was fit for work. In a second instance, the boiler—also recently enrolled—proved to be nearly corroded through in several places in consequence of leakage from joints which were bolted instead of being riveted. In a third instance, a two-flued boiler, 7 ft. in diameter, was found to be so corroded that the plates in one of the external flues were reduced to about one-eighth of an inch in thickness, which is a most serious reduction for a boiler of such dimensions. In a fourth case, the under side of the furnace flue of a Cornish boiler was so weakened by internal corrosion that the inspector made a hole through the plates on sounding them externally, *i.e.*, from the inside of the flue. This being a single-flued boiler, and the corrosion having taken place at the bottom of the furnace tube, the injury could not be seen, even on an internal inspection, and therefore was liable to escape detection; this is not the case with a "Lancashire" boiler, in which the bottom as well as the top surface of the furnace tubes are accessible to examination. In a fifth case, a boiler, which had not been thoroughly examined for upwards of two years, was found to be most seriously affected internally by the action of corrosive water. Many of the rivet heads were half eaten off, and presented a spongy appearance, the plates were extensively honey-combed, some of the indentations being as much as one-eighth of an inch deep, and others on the furnace crowns a quarter of an inch; while the longitudinal stays were eaten into holes and in some parts reduced to half their original strength, as well as the cotters seriously affected; in addition to which one-half of each of the internal flue angle irons was eaten away, some of them being brought up almost to a feather edge. It is scarcely three years since the boiler was new; while continuance of such service without attention would soon have rendered it unfit for work altogether. In cases of acidity in the feed water, simply soda is found most effective, and had been recommended by the Association in the present instance, the corrosive action being suspected from the appearance of the water in the glass gauge, though its effect on the boiler remained unknown till the thorough examination was made. In a sixth case, grooving or furrowing action proved to have worked its way through the solid plate within the thickness of a sheet of paper.

These cases which are not peculiar, and were all met with during the last month's inspections, will suffice to show the importance of the members affording an opportunity for each of their boilers to be "thoroughly" examined at least once every year. In order, however, that these examinations should be satisfactory, it is imperatively necessary that the flues should be well swept and the plates brushed. When several inches of soot remain in the flues, and the plates are at the same time coated, it is impossible for the inspector either to do himself or the examination of the boiler justice, and simple as the preparation of boilers for "thorough" examinations may appear, yet it has so important an influence on the value of the inspection, and at the same time is so frequently neglected, that the attention of the members is once more called to the subject.

Advantage has been taken of the past Christmas week to make as many "thorough" examinations as possible, and it may be stated that it is important, in order to prevent disappointment, that those members who desire to avail themselves in this way of these general holiday times, such as Easter and Whitweek, &c., should make early request for inspection, since it is impossible at a few hours notice—as is too frequently expected—to alter arrangements that have previously been made for days.

Explosions.—No. 18 Explosions occurred to one of a pair of marine boilers, not under the charge of the Association, and working on board a tug-boat. They were not multitubular but flued boilers, each having two furnaces which united in a single return flue and then passed off to the uptake.

The explosion was caused by the collapse of the outer or wing furnace crown. of the starboard boiler. The furnace crowns, though not entirely semicircular, were arched elliptically, being made of plates of seven-sixteenths of an inch in thickness, and worked at a blowing-off pressure of 28 lbs. The crown of the wing furnace came down till it rested on the bars and rent at a transverse seam of rivets immediately over the fire, the adjoining furnace being also slightly depressed but not rent and no other part of the boiler damaged. The fireman, who was the only one below deck at the time, was scalded to death.

This boiler was not personally examined, but the collapse is reported to have been due to shortness of water, and there does not appear on consideration of all the circumstances, to be any reason to differ from this conclusion.

No. 26 Explosion.—The boiler in question, which was not under the inspection of this Association, was of plain cylindrical egg-ended construction, and externally fired.

Its length was 30 ft., its diameter 6 ft., and thickness of the plate one-half of an inch. The longitudinal seams of rivets were arranged so as to break joint, the boiler being well made throughout and new about nine months previous to the explosion. while none of the plates appeared to be weakened by corrosion neither was there any evidence of leakage at the seams. The boiler was liberally supplied with the fittings usually to its class, having duplicate floats with alarm whistle, duplicate lever weighted safety-valves, perfectly open and a feed back pressure-valve, &c., while the blowing-off pressure was 55lbs., which was by no means excessive for such a boiler.

On examination of the fragments of the shell, it was found to have separated into four distinct pieces, both of the dome ends with one or two rings of plate, having torn away at the transverse seams, while the remainder of the cylindrical portion of the shell had rent longitudinally, and also separated into two pieces.

The flight of the dome ends, which had taken place in opposite directions, had been productive of considerable injury; the one at the firing end flying out of the works, crossing an adjoining street and falling against a dwelling-house on the opposite side, and thereby very seriously injuring one of its inmates, but fortunately not fatally so; while the other dome end of the boiler, skated as it were along the ground, sweeping down a great deal of building in its course, so that a considerable portion of the works was reduced to a heap of ruins, and the *debris* scattered in every direction.

In stating the cause of the explosion, it should be pointed out, that the boiler though externally fired was set upon a mid-feather, up one side of which the flames passed and then returned on the other; while the flues were carried up to a level with the centre of the boiler, to which height, therefore, the flames could act upon it. The flame passed on the right hand side of the mid-feather in the first instance, and it was just at the top of this flue and a little beyond the bridge, where the flame would be in full force, that the primary rent, which was a longitudinal one, appeared to have occurred in the shell of the boiler. This naturally awoke the suspicion that the water level had been allowed to fall below the top of the flue, and this view was strengthened by an examination of the plates, which both from their color, as well as from the character of the rupture, appeared to have been overheated; while in addition, there were water lines plainly marked on the inside of the boiler considerably below the top of the flues, so that it appeared highly probable that the water had not been allowed to run short for the first time on the day of explosion. The boiler attendant acknowledged that he had not tested the floats to see if they were free, for about four hours previous to the explosion, so that there seems no room to doubt, that it was due to shortness of water; which, however, it may be added, is the only instance of the kind that has been met with during the past year, in those explosions personally investigated by this Association.

No. 27 Explosion was the result of the old age and neglected condition of the boiler in question, which was not under the charge of this Association, while fortunately no one was killed by the explosion, and only one person injured.

The boiler was but small and of plain cylindrical egg-ended construction externally fired, being only 12 feet long and four feet in diameter; the plates varying in thickness, some being seven-sixteenths and others three-eighths of an inch. The pressure was stated to have been 25 ℔s. per square inch, but there had been no steam pressure-gauge, and the fittings were altogether of a meagre description. The boiler was plated so that the longitudinal seams did not break joint, but were in line or nearly so, some of them falling immediately over the fire, and it was at one of these which had been clumsily patched, that the boiler ruptured, the rent extending from one end to the other, and the shell completely opening out.

Its flight was somewhat remarkable; it took down the chimney in its course, flew over a row of dwelling-houses, as well as a public road and a canal, landing finally in the bed of a river, immediately under one of the arches of a railway bridge which ran across it.

As to the cause of the rupture, the boiler was at least 25 years old had been working unroofed, and thus exposed to the action of the weather, and was found upon examination to be seriously corroded both inside and outside, having been patched in several places in consequence. This corrosion had eaten off the heads of many of the rivets, and seriously reduced the thickness of the plates, especially at the longitudinal seam over the fire at which the rupture had taken place; so that this explosion is simply due to the dilapidated condition in which the boiler had been allowed to fall through old age and neglect.

No. 34 Explosion.—The boiler in question was not under the inspection of this Association. It was internally fired and of single flue or Cornish construction, its length being 23 feet, the diameter of the shell 5 feet 6 inches, that of the furnace 3 feet for a length of 7 feet, and in the flue 2 feet 6 inches, while the thickness of the plates was five-sixtenths of an inch, and the pressure at which the steam blew off 40 lbs.

The furnace flue, though reported to have been amply covered with water at the time, collapsed immediately over the fire at about 2 feet 6 inches from the front, which admits of no surprise considering the diameter of the flue, the lightness of the plates and the pressure of the steam, so that this explosion can only be regarded as one of those already so numerous, which might have been prevented by simply strengthening the furnace flue with any one of the inexpensive means frequently called attention to in these reports, viz: flanged seams or hoops, whether of ⊤-iron, angle or other suitable section, as well as by the adoption of other approved methods.

The boiler was not moved from its seat by the explosion, and the glass water-gauge and steam pressure-gauge remained uninjured, but owing to the scalding effects of the steam and hot water which escaped from the rent, as well as to the flight of the furnace mountings and other fragments which were shot away, three persons were killed and two others seriously injured.

No. 40 Explosion.—This is the second instance which has occurred within the last few months, of a new boiler exploding at the makers yard while being tested with steam, and which might have been prevented by the simple use of the hydraulic test. Two men were killed and seven others injured by the explosion of this boiler, which was not under the inspection of this Association.

The boiler which was intended for a tug-boat was of the flued class, internally fired, having two furnaces, and being very similar to that described under explosion No. 18, while it had a large steam dome on the top through which the flue passed on its way to the chimney.

The shell rent along the top longitudinally, and was completely opened out and laid flat upon the ground, in which position it, as well as the flues which were uninjured, remained unmoved, while the steam dome was torn completely away and shot upwards, being blown over some high buildings into an adjoining yard.

The fact of the steam dome being torn off, and the rent starting from it on the top of the shell, as it appears to have done, clearly shows the dangerously weakening effect that these large openings have upon boilers, so that there can be no doubt that this boiler was of defective construction, and that this fact might have been detected in time, the explosion prevented, and the lives saved, had the hydraulic test been adopted.

No. 41 Explosion, which resulted in the death of both of the engineer and fireman, occurred to a boiler not under the charge of this Association, and at too great a distance from Manchester to be personally investigated, while considerable difficulty has been experienced in gaining authentic information with regard to it.

It appears that the boiler was one of two of Cornish construction connected together and working side by side, and that the internal furnace flue collapsed from end to end, rending across three of the transverse seams of rivets.

The collapse was attributed to the boiler having been allowed to run short of water, the jury giving it as their opinion that "The deceased men died from scalds caused by the bursting of a tube in the boiler, arising from want of water."

The explosion occurred at about six o'clock in the morning, the boiler being worked night and day, and it appears that the night fireman had been drinking and thus neglected his duty.

No. 42 Explosion resulted in the death of four persons as well as injury to three others. The cause of the explosion of this boiler, which was not under the care of this Association, has been made the subject of an official scientific investigation, and as soon as a copy of the report is received, which is promised shortly, the particulars will be communicated to the members.

It may however be stated in the meantime, that the explosion has been generally attributed to shortness of water, but it appears from information kindly afforded by an engineer, who made an examination shortly after the explosion, that the furnace flue of the boiler—which was a Cornish one—was seriously out of shape and unfit for work; so that the explosion seems to have resulted simply from weakness of the flue and not from want of water.

No. 43 Explosion, from which one person was killed and four others injured, was in many respects very similar to No. 41, being due to collapse, which extended throughout the entire length of the furnace tube of an internally-fired single-flued Cornish boiler, which was not under the charge of this Association, while the distance at which the explosion occurred precluded a personal investigation. It appears, however, from a report obtained that the length of the boiler was 20 feet 6 inches, its diameter 5 feet 6 inches in the shell, and 3 feet 1 inch in the furnace flue, which was made of plates three-eighths of an inch in thickness. Only one of the two safety-valves with which the boiler was fitted was in working order, while the ordinary pressure of the steam was stated to have been 49 lbs. on the square inch, but what it may have been at the time of the explosion was not actually known.

A furnace flue of such dimensions as the above might when new, if of good materials and well put together, as well as of true circular shape, work for a time at a pressure of 40 lbs. per square inch, but—taking into account imperfections in workmanship in the first instance, the effect of several years, wear and tear, and the frequent excess of the actual over the supposed pressure of steam, which is frequently found to exist through errors in safety-valves and steam pressure-gauges—it certainly is not prudent to work within so narrow a margin of danger, especially when safety could be insured simply by hooping the flues; and although the collapse was again in this instance attributed to shortness of water, it is thought to be rather to the neglect of the above precaution, that the explosion is really due.

During the past month three explosions have occurred, from which five persons have been killed and four others injured; not one of the boilers was under the inspection of this Association. One of these has been personally examined subsequently to its explosion, while distance prevented this being done in the other cases. The following is the monthly tabular statement:—

Tabular Statement of Epxlosions, from November 21, 1863, to December 31, 1863, Inclusive.

Progressive No. for 1863.	Date.	General Description of Boiler.	Persons killed.	Persons injured.	Total.
44	Nov. .	Double furnace "breeches" boiler Internally fired. . . .	0	0	0
45	Nov. 23.	Cylindrical egg ended, Externally fired. . .	1	4	5
46	Nov. 27.	Ordinary single flue or "Cornish" Internally fired, . . .	4	0	4
		Total, ...	5	4	9

No 44 Explosion occurred to a boiler not under the inspection of this Association, and at too great a distance from Manchester to admit of a personal investigation being made, but the following particulars have been kindly furnished by an engineer who examined the boiler shortly after the explosion took place.

It was a breeches boiler, internally fired, having two furnaces running into one flue. Its length was 15 feet and diameter 6 feet, while the ordinary working pressure was 60 lbs.

The boiler rent along the bottom longitudinally from one end to the other, just as if it had been set upon a mid-feather, and the plates thereby corroded away; but it proved on inquiry to have been but six months old, and set upon two side walls, so that a longitudinal rent under these circumstances is peculiar. The cause of the rupture appears to have been as follows:—A heavy cake of deposit, chiefly of salt, and several inches thick, had through neglect been allowed to form inside the boiler along the bottom. This, under ordinary circumstances, would have not been attended with any dangerous consequences in an internally-fired boiler, since the temperature in the external flues is not usually sufficiently high; but in this case the conditions were peculiar; the boiler as will be seen by the dimensions given above, was an unusually short one, at the same time that the draft was good, and the furnaces heavily fired. Since, therefore, the course of the flame passed under the bottom of the boiler immediately after leaving the internal flue, it is not difficult to understand how the plates at the bottom of the shell, which were heavily coated internally with sediment, became overheated and then rent. Surface blowing out would, in all probability, have prevented this explosion.

It will be seen that the circumstances of this boiler were such that it was virtually fired internally and externally at the same time, and it therefore affords an apt opportunity for comparing the safety of the two systems of firing, and it will be noted that the explosion was not due to that portion of the boiler which was fired internally, but to that which was fired externally.

It was not only fortunate for the two men standing in the stoke hole at the time of the explosion, but it is also an interesting engineering fact, that sufficient warning was given them to make their escape, one of them hearing the rush of the water directly the rent commenced, and giving the alarm, while the boiler lifted from its seat and flew through the roof an instant after. This fact is an important one, since it shows, that in this instance, at all events, no explosion like that of gunpowder took place, though such is generally supposed to be the case, but that the result was due simply to the pressure of the steam acting according to its ordinary laws, and to the quite rending of the plate.

No. 45 Explosion.—This boiler was not under the inspection of this Association, neither was it personally examined after its explosion, which, however, is reported to have been the result of almost unparalleled recklessness.

The boiler was a stationary one of plain cylindrical egg-ended construction, and externally fired, being 16 feet long, 3 feet 6 inches in diameter, and made of plates three-eighths of an inch in thickness. It was employed for agricultural purposes, and under the charge of a fireman as well as of a superentending foreman or farm bailiff. The engine which it drove had lately undergone repair, and steam was being got up for a fresh start, when the foreman, in spite of remonstrance, persisted in keeping the safety-valve down by sitting on the lever, the carpenter being engaged in the meantime, by the foreman's orders, in cutting a wooden prop to jam the valve down. Before, however, the prop could be obtained, the boiler exploded, instantly killing the foreman and severely injuring the engine-man and the other persons present.

The boiler was completely severed into two parts, one portion being blown to a considerable height, and thrown to a distance of 150 feet, the rent passing through the manhole and the greater part of the second transverse seam of rivets from the front end, which was situated immediately over the fire. The position of the safety-valve on the boiler was such, that the man whilst sitting upon the lever was immediately over the manhole, and both he and the cover, were blown by the explosion in the same direction, and picked up at a considerable distance from the original seat of the boiler.

With regard to the cause of the explosion, there is no ground for concluding that the steam, although the valve was held down, had time to attain to the bursting pressure of a boiler of such dimensions as this one was, supposing it to have been in good condition, and there seems, therefore, reason to think that the boiler had been somewhat weakened by the twelve years service it had seen, although there can be no question that the immediate occasion of the explosion was excessive pressure caused by recklessness such as these reports have not previously had to record.

No. 46 Explosion, which resulted in the loss of four lives, occurred to the right hand boiler of a series of three working side by side and connected together. These boilers were not under the inspection of this Association.

The one which exploded, as well as that next to it, was of Cornish construction, being internally fired, and having a single flue running through it, which diminished in diameter behind the fire-bridge. The length of the boiler was 26 feet 3 inches, the diameter of the shell, 8 ft. 3 ins. that of the furnace tube 5 feet for a length of 8 feet, and of tho flue, 2 feet 9 inches, while the thickness of the plates throughout was scarcely three-eighths of an inch, being especially bare in the furnace.

The boiler had been fitted with a feed back pressure-valve, a brass blow-out tap, and a glass water-gauge, as well as with an ordinary lever safety-valve and a steam pressure gauge common to boilers No. 2 and No. 3. The pressure at which the safety-valve was stated to have blown off was 40 lbs. by the gauge, but on calculation it appeared that this should be taken as minimum, and that with the steam blowing-off freely the pressure would no doubt rise as high as 45 pounds, if not 50 pounds.

On examining the boiler, the furnace tube was found to have collapsed immediately over the fire, and rent at one of the transverse seams as well as at its attachment to the front end plate. The collapse had not extended into the flue behind the fire-bridge, or affected the conical junction piece between the flue and the furnace tube.

As to the cause of the collapse there cannot be a doubt. No flue 5 feet in diameter, made of plates barely three-eighths of an inch in thickness, can be safely subjected to a pressure of 40 lbs. or 50 lbs. on the square inch, as this one was; and hence the explosion. The boiler was an old one, and though not materially weakened by corrosion, had evidently been made at a time when the use of high pressure steam was very little understood. The cylindrical portion of the shell was stayed transversely with horizontal bolts both above and below the furnace tube, as well as with others running vertically on each side of it, while the flat ends were stiffened with diagonal stay bolts. No attempt, however, was made to strengthen the large furnace tube, though the weakest part of the whole boiler, while the diagonal rods introduced, are very inferior to the gusset stays now in use, and the horizontal and vertical ones afford but little or no support to the cylindrical portion of the shell.

The personal injuries due to the explosion arose from scalding, while the effects on the surrounding property were but trifling, which is frequently found to be the case when explosion results from collapse of the flue of an internally fired boiler. In this instance the boiler was not stirred from its original position, nor the joints of the steam pipes broken, and in order to be in a position to work on with the other boilers, it was only necessary to screw down the junction valve. The principal injury to the surrounding property resulted from the flight of the furnace mountings, which in this case were unnecessarily heavy, and included a facing of brick-work, which is always objectionable in such a position. It will thus be seen that in the case of collapse, the weight of the furnace mouth-piece becomes a source of danger, and the fact of damage frequently arising from this cause, and in some cases loss of life, is an argument for the adoption of light wrought iron furnace mountings, which are far superior as a mechanical arrangement.

The insufficiency of this boiler for the pressure at which it was worked could not have escaped detection either on competent inspection, or the application of the hydraulic test.

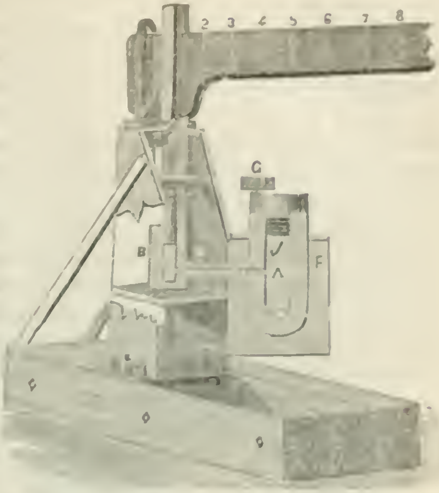
There can be little question that many explosions occur in different parts of the country of which no report is obtained by this Association, but it will be seen from the tabular statement above that 46 came under its notice during the year 1863. The number of boilers which exploded, however, amounts to 50, since an entire series of 5 working side by side was included in one explosion; while during the year there were 76 persons killed, and 77 others injured.

It is thought that a tabulated statement of these explosion, giving briefly the cause of each, with a general description of the boiler in every case, would be of interest, and therefore it is proposed to give this in the Annual Report for the year 1863, which will be presented to the subscribers at the ninth annual meeting now shortly to be held; and it need merely be added, therefore, in the meantime, that a review of the explosions which have occurred during 1863, clearly shows the value of periodical boiler inspection, and supports the principle advocated in the previous reports, viz:—that the ruling cause of boiler explosions is neglect, and their preventive, simply, care.

Thickness of the Earth's Crust.—Extract from a Lecture by W Fairbairn, Esq.

From the London Mechanics' Magazine, May, 1864.

It has always been a question of interest to geologists to ascertain the thickness of the earth's crust, or at what depth under the surface it becomes fluid. To determine, or rather to approximate, the solution of this question, my friend, Mr. Hopkins, of Cambridge, Dr. Joule, and myself, were requested by the British Association, to institute a series of experiments on pressure and the conductivity of the different strata with which we are acquainted. This very interesting inquiry has extended over a series of seven to eight years, and the results obtained are of that nature which, I trust, will justify me in bringing the subject before you. The whole of the experiments have been carried on at my works in Manchester, having, at the time, a powerful apparatus, as shown in the engraving.



A is a strong cylinder of brass, with a branch communicating with the cylinder B, into which works a steel plunger *c*, which fits into the socket in the bar D. On the top of this bar the powerful lever E is applied, which, pressing on the liquid in the cylinder B, forces it through the bore of the pipe *a* (on the principle of the Bramah), and by which nearly any amount of pressure may be attained. On the top of the cap of the cylinder A, which is made air and water-tight and inserted in the bath F, a mariner's compass G, is placed, the use of which we shall presently describe.

Having adjusted the apparatus, and filled the cylinder A with the material to be experimented upon, a charcoal fire is lighted under the bath, and before applying the lever the point of the fluidity is ascertained. This was accomplished with great difficulty in the first instance, as we had no means of ascertaining, with anything like accuracy, the melting point, until Professor W. Thompson, of Glasgow, who happened to be present at one of the experiments, suggested the compass, which answered the purpose admirably. This ingenious application was effected by inserting a magnetic needle in the solid mass to be experimented upon, and, having brought the needle of the compass within its influence, it pointed directly over the centre of the cylinder, and was retained in that position so long as the material was in the solid state; but the instant the melting point was arrived at, by increasing the temperature of the bath, the needle, no longer supported by a solid substance, dropped through the fluid to the bottom of the cylinder, and, the instant that was accomplished, the needle of the compass turned round to the pole. From this arrangement it will be seen that the temperature of fluidity was carefully and accurately determined.

Leblanc's Water Gauge.—Patented by DESIRE FRANÇOIS LEBLANC.

From the London Mechanics' Magazine, September, 1853

We feel much pleasure in laying before our readers particulars of a glass water-gauge which is extensively used in France, and deserves general introduction here. It is not only stronger and safer perhaps than any other gauge which shows the water level through a transparent medium, but it possesses the great advantage of always denoting the presence or absence of water from the glass tube. The ordinary gauge may be quite full yet appear empty, or quite empty and appear full. It is impossible that such a result can take place with Leblanc's gauge, for reasons which the specification will best explain:—

Let us consider a rather thick-sided glass tube half-filled with water we shall readily note the wide difference in refraction which exists between the full portion of it and the empty part thereof; and now let there be a screen provided with a circular hole of about two-thirds of the outside diameter of this tube placed behind that portion of it which is full of water, and this circular hole will appear in the form of an ellipse, having its great or transverse axis perpendicular to the axis of the tube, whilst on the same screen being removed behind the empty portion of the tube the ellipse assumes a rather lenghtened form, having then its great or transverse axis parallel to that of the tube.

It follows from this, first, that such a screen may be used to inform us—when no solution of continuity appears in the tube—whether this is quite empty or quite full.

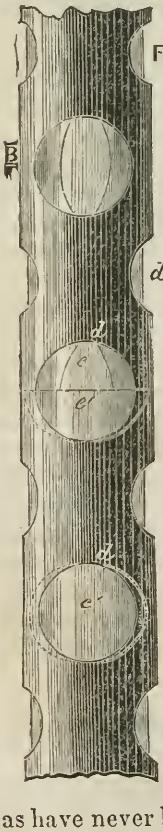


FIG 1.

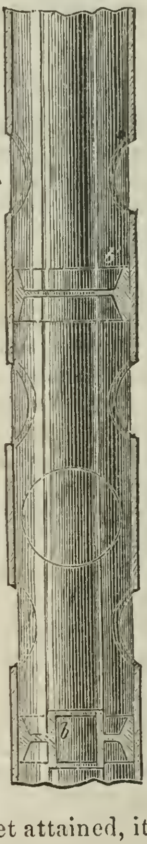


FIG 2.

And secondly, that however darkened its sides may be, unless they are altogether opaque, it will always be possible for any one to ascertain with its aid the presence or the absence of the liquid inside.

A metallic tube pierced from end to end with circular holes opposite to each other on two or three alternate diameters and encasing the glass tube, may therefore become at once both a protection against breakages and their effects, and a means of more distinctly showing the height of the liquid inside.

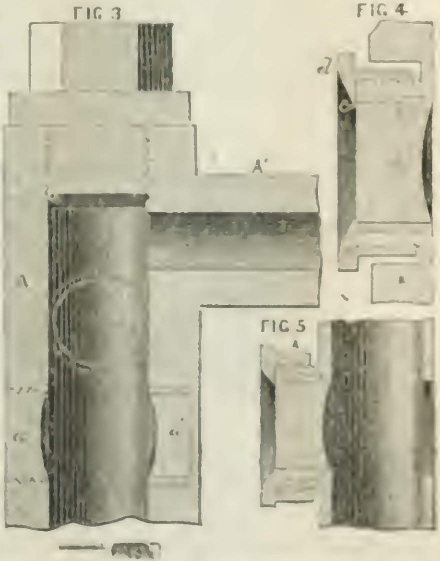
Such is the nature and object of the said invention for "A Dioptical Metallic, Level Indicator or Liquid Gauge," but essentially consisting in the application of the combined dioptical properties of glass and water to the construction of a liquid gauge that should possess such requisites to clearness of indication and security of service as have never been as yet attained, it is susceptible of any form; for example, a glass tube consisting of several short ones may be advantageously substituted for that of one piece, and lenses for either of them.

Again, a casing made of any flexible matter, as vulcanized or hardened india rubber, or simply sheets of paper or cloth, impregnated with siccative greasy substances, rolled several times and stuck on the glass tube, may also be used instead of the metallic casing.

Being given a quadrangular opaque tube, the sides of which are pierced with two circular holes opposite to each other; if you fix in each of them a plano-concave lens, whose concavity faces the axis of the tube, and whose focus lies a little behind the opposite side; the hole that faces the lens which is looked at will appear greatly reduced in diameter, the tube being empty; whilst on the same being filled with water, the index of refraction of this being pretty nearly the same as that of glass, the concavity of the lens is found to be almost annulled by the convexity of the water, and the opposite hole re-appears with its nearly full diameter. The crown glass gives the maximum of differential effect.

Figs. 1 and 2 of the foregoing drawings represent a glass tube enveloped in a metallic casing pierced with circular holes opposite to each other. The metallic casing may be simply a non-adhesive case surrounding the glass tube, but it is better to have it fixed thereon, by means of a mastic or cement, such as that of ceruse and minium, oxychloride of zinc, and such like.

A is the glass tube; B, the metallic casing; *c*, the mastic; *d*, the holes of the casing; *e*, the outline and direction of the ellipse, as seen in the empty part of the tube; *e*^{1}, the same as seen in the part occupied by the liquid.



Indicators of unlimited length and best fitted to resist expansion will be obtained by using short glass tubes joined end to end. For this purpose it is necessary that each joint should correspond with the middle of a space without holes, so long as the sealing should prevent leakage.

The mastic of ceruse and minium, by reason of its solidifying slowly in a cold place, appears to be the most suitable one to employ. The complete solidification is obtained by its being exposed for two or three hours to a temperature of from 200 degs. to 250 degs. centigrade. All mastics that solidify slowly in a cold place and quickly in a hot one may likewise be advantageously used. Colored mastics, and above all those colored with black, render more distinct the perception of the optical phenomenon.

The glass tube extends a little beyond the metallic casing, in order to obtain a hermetical joint, in case the sealing of its extremities to its casing should not be quite water-tight, and thus to prevent acids and other agents from attacking the metal or the mastic. This disposition, however, is not indispensable, and in most cases the glass tube will be level with its casing.

Fig. 3 represents the section of the indicator, provided with lenses.

A, metallic body; A', tubulatures, capable of receiving cocks; a, a', a'', plano-concave lenses. There are many means of fixing the lenses in the holes; they may be screwed with an oily mastic, as in a', or simply sealed with a mastic, specially that of cast iron in brimmed holes, as in a'', or only hot fixed by means of a sufficiently fusible enamel, provided that the glass made use of be of a dilatation pretty nearly the same as that of the metal employed, which should be little dilatable itself, such as cast iron, iron, steel. I do not mention platinum because of its high price.

Lenses may also be placed on thinner tubes by means of tubulatures, A, fig. 4, into which they are fixed, either with a mastic or with india rubber, by means of a screw cap. They may likewise be sealed with an enamel into very thin tubulatures which will attenuate the effects of expansion. By enamelling the cast iron, iron, and steel tubes, they will thoroughly be kept from oxidation.

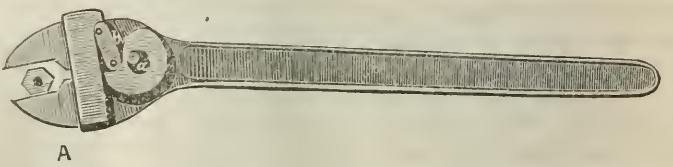
Fig. 5 represents an enlarged view of a mode of fixing the lenses in the tubulatures.

A, lens; B, side of the tube; c, externally screwed tubulature; c , matter of the joint, which matter may be, according to the case, either a ring of vulcanized india-rubber, or amianth, impregnated with mastic or simply oiled hemp for both low and mean pressures; c'', metallic ring that compresses the joint; d, screwed cap, screwing on the metallic ring.

Self-Acting Spanners or Screw Wrenches.

From the London Artizan, June, 1864.

These spanners are a contrivance of Mr. L. Schwartzkopff, of Berlin, whose steam hammer we illustrated in *The Artizan* of September, 1862.



The main feature in these spanners is the simplicity of their construction, as they can be used for nuts of all sizes by simply applying them to the nut and turning a hand-lever, without any special mechanism for *setting* them being required. The chuck a is fixed on the hand-lever, and connected with the small lever b, which is set in motion by the movable jaw A being pressed against one side of the nut. By this means the spanners will close themselves, irrespective of the gauge of the nut, so long as the latter can be contained between the two jaws.

Refrigerating and Ice-Making Machines.

Report on the apparatus by M. Carré, having for its object the production of artificial cold.

(Commission composed of MM. Regnault, Balard, and Pouillet, reporter.)

From the Practical Mechanic's Journal, October, 1863.

The following valuable Report of a Commission of the Institute of France, upon M. Carré's ice-making machinery, presents many considerations more or less generally applicable to all machines of this class; and as placing some of the more fundamental points relating to these machines, as yet so imperfectly understood by mechanical engineers in this country, in a very clear light, we have deemed it fitting to supply our readers with a translation of the entire report.

M. Carré has presented to the Academy the drawings and description of an apparatus which he has invented for producing cold, and for solving the problem of artifical cold in all its generality, whether it be proposed to make ice, or simply to cool large volumes of air or liquid, or finally to cause very considerable masses of liquid to fall to the low temperatures of 15° C., (5° Fahr.,) or 20° C., (4° below zero, Fahr.,) for he is endeavoring to apply this invention to producing great degrees of cold in the sea water of some extensive salt works in the south of France.

The Commission met several times in the workshops of MM. Mignon and Rouart, where there was a model of this apparatus. They examined its construction, and further, they made it work under their eyes, as often and as long as they wished, in order to appreciate its work.

This model had dimensions large enough to render it possible to take into account all the physical and mechanical phenomena which conjoin to the result, and which insure the regularity of its working for whole days together.

The most of its operations are for the purpose not of cooling brine, but of making ice; and at its ordinary rate of working, the apparatus gives 25 kilogrammes an hour or 250 kilogrammes a day. Its refrigerating power then was 2500 units of heat (calories) an hour, since water, taken at ordinary temperatures, must lose nearly 100 units per kilogramme in its transformation into cylinders of ice, the very interiors of which are several degrees below zero.

In estimating, as we have just done, the power of the apparatus by the number of units of heat which it is capable of abstracting per hour from the body which is given to it to cool, it must not be lost sight of that there is still another element which must be taken into the account namely, the *intensity* of the cold produced. In fact, an apparatus which takes 2500 units per hour from a body in causing it to descend, for example, from 10° (50° Fahr.) to 0° (32° Fahr.,) is in no way comparable to another apparatus which takes the same 2500 units per hour but makes it descend from 20° to 30°. It is absolutely necessary then, to express in a complete manner the refrigerating power of a given apparatus, to mention at the same time the number of units which it abstracts per hour, and the two limiting temperatures between which the cooling is accomplished.

The general principles on which the construction of the apparatus of M. Carré rests are very simple. They are put in practice in the various cryophores and refrigerators hitherto invented. They must be here referred to.

A more or less volatile liquid is contained in an hermetically sealed vessel, analogous to a steam boiler, but with this difference, that instead of receiving the heat of the fire, it gives out cold all round it. This vessel, which we shall call the refrigerator, communicates, by a large tube and stop-cock, with an empty space, which we shall suppose at the commencement to be very large. At the moment of opening the stop-cock, the vapors of the liquid rush into the void by their elastic force, other vapors form on the instant, which rush in in their turn, and the operation thus goes on as long as there is any liquid to vaporize. These vapors cannot form without taking from the walls of the refrigerator all the latent heat which is necessary for their existence and their elasticity. Thus the refrigerator becomes cooled more and more, and to establish an equilibrium temperature, it abstracts from the bodies which touch and surround it the whole quantity of heat which it has had to furnish for the evaporation. If there is formed, for example, according to the capacity of the refrigerator, 10 or 100 kilogrammes of vapor an hour, the number of units abstracted will be

5000 or 50,000, if the volatile liquid is water;
2000 or 20,000, " " alcohol;
900 or 9,000, " " ether;

for the latent heats of these liquids are about 500, 200, and 90 units per kilogramme.

As to the degree of cold to which the refrigerator can reach by these spontaneous evaporations, it depends principally on the nature of the volatile liquid. By supplying it with water, we with difficulty get to a few degrees below zero, because solidification without wholly depriving it of the property of giving off vapors, does deprive it of the property of giving them off in great abundance. On the contrary, alcohol, ether, and other volatile bodies which remain liquid at very low temperatures, continue also capable of giving off abundant vapors, and consequently of producing very great degrees of cold.

What is thus so simple in theory becomes strangely complicated when it comes to be put in practice, when it is necessary to embody these primary ideas, in such a way as to build up a great continuously working machine, working with regularity, and governing itself almost like a steam engine. This was truly a difficult question, and M. Carré has at last given us a satisfactory solution of it.

Let us first point out the principal difficulties, or rather the points on which they bear.

1. We have supposed the refrigerator was put in communication with an empty space, indefinitely great, and that the vapor could form without ceasing, in virtue of the elastic force which is proper to it.

This hypothesis is not realizable. We must then recover this vapor as fast as it forms, and further compress, liquify, and collect it, to employ it again, because it would cost too much if it were allowed to be lost.

2. It is necessary to introduce into the refrigerator a weight of liquid equal to the weight of vapor which forms in it in a given time, and which can be pumped out of it, as we have just said. This is the rigorous condition, without which the action of the apparatus could not be regular or continuous.

3. It is necessary that all the joints and closings should be hermetically tight. The smallest quantity of air penetrating to the interior would suffice to compromise all, and the same would happen if the vapors could escape outwards.

4. In proportion as the limiting temperature at which the refrigerator ought to be lowered, the elasticity of the vapor decreases, and the volume which an equal weight occupies becomes greater and greater. Accordingly, when we want to form a given weight in a given time, for instance, 10 or 100 kilogrammes an hour, it is plain there will be a whole series of experiments to make on the forms and dimensions to be given, not only to the interior of the refrigerator, but to the supply and delivery pipes, the cocks, and in one word, of all the parts which assist in the formation or circulation of the vapor.

5. Finally, although certain complex liquids, as the solution of ammonia in water, present certain marked advantages in some respects, they are subject to all the preceding difficulties, and besides, are subject to difficulties of another nature, depending on the two vapors which they form, besides the necessity of regulating the variable proportions of their mixture.

The apparatus with which we are occupying ourselves in fact contains an ammoniacal solution as the liquid for producing the cold, and we ought consequently to expect to find in it all these difficulties at once.

Accordingly, we must attempt to give some idea of its construction, at least as far as we can without the aid of figures.

The ammoniacal solution undergoes four changes of state.

It is vaporized by a boiler.

This vapor is condensed by a liquifier. In this new state the liquid is received into a distributor, which introduces or distributes it in proper quantity into the refrigerator.

Here the liquor evaporates afresh, to produce the cold.

6. These new vapors are withdrawn through a large tube, and condensed in an absorbent reservoir, where they are exposed to the impoverished liquid drawn from the boiler itself. The poor liquid becomes rich by the absorbtion of the ammoniacal vapor, and submitted to the double effect of an exhausting and condensing pump, which exhausts it from the bottom of the absorbent reservoir, to condense it in the boiler from which it started, partly in the state of vapor, and partly in that of liquid.

Thus every thing is reduced to a complete circulation of the volatile liquid, of which the two elements, water and ammonia, are alternately separated and re-united, either by condensation or evaporation, their mutual affinity playing here an important part, which is worthy of remark.

In order better to understand how this circulation goes on indefinitely and always with the same original quantity of liquid, we shall separate it into two parts, namely, *The passage from the boiler to the refrigerator* and *The passage from the refrigerator to the boiler.*

Passage from the boiler to the refrigerator.—In the model of the size for 2500 units per hour, the work of which we have been enabled to observe, the boiler is a vertical cylinder, of 1 m. 20 c. m. high, by 0 m. 40 c. m. in diameter. With a capacity of 1 hectolitre and a half, (=3·2 gallons nearly) it is charged with only 80 or 90 litres of a very concentrated solution of ammonia. It is maintained at a temperature which does not exceed 130°, (266° Fahr). Thus the tension of the united vapors of water and of ammonia is that of 8 atmospheres.

The upper half of the boiler is outside the furnace, and in contact with the air. It is furnished inside with a series of vessels, superimposed on one another, forming a sort of rectifying cascade, where the ammoniacal vapor frees itself in great part of the watery vapor which it contained.

This dried vapor escapes by a long tube of suitable section, which conducts it to the connector (chevet) at the entrance into the liquifier.

The liquifier is composed of four worms, plane and parallel, placed 5 centimetres from one another. The tube of each worm opens into the entrance connector, which is horizontal. It then continues in a right line, to a length of 1m.50, with the slope necessary for the flow of the liquid; there it curves round to return, always descending in the same vertical plane, makes a second ply, and again a third ply similar to the first, at the end of which it opens into the exit connector, which is parallel to that at the entrance. This system of zigzag worms is plunged in a great vessel of cold water, which is renewed sufficiently to prevent the temperature rising to 30° (86° Fahr.) by reason of the condensation of the vapor which takes place inside the tubes.

The exit connector of the liquifier receives also all the liquid which has been formed in the worms, more from the effect of the cooling than from the effect of the pressure of 8 atmospheres of the boiler, a pressure which transmits itself directly, and without any break, up to the point at which we have now arrived. Here the transformation is complete, and for the rest of the passage, and up to the refrigerator, it is liquid that circulates; but it is not the less subject to the pressure of the boiler, because it has no further obstacle to modify its free progress.

This liquid must not arrive at the refrigerator except in the most exact proportion, and with perfect regularity; hence a distributor is necessary, which shall regulate its supply.

The distributor is a cylindrical vessel, of 25 or 30 centimetres high, having a capacity of 4 or 5 litres, and carrying near the top a lateral tube for the entry of the liquid. A pipe starts from the bottom of this vessel, and is prolonged downwards, and with a common axis with the cylinder. It is 15 or 20 centimetres long, and only 2 centimetres interior diameter, except at the bottom, where it is contracted for a few millimetres, and ground so as to make a sort of valve seat. There it is closed, and carries on the side, near the middle of the height of the valve seat, a small opening for the exit of the liquid.

A light and delicate float, open above and closed below, with the exception of a hole which corresponds with that of the valve seat, can move freely in the vessel, with which it corresponds in form, except that it is a little narrower, and only touches it for the height of the valve seat. The whole movement of the float is reduced to a vertical oscillation, which cannot exceed 10 or 12 millimetres, and which it performs without being able to turn on its axis.

The following is the manner in which this ingenious distributor acts. A tube establishes free communication between the exit connector and the interior of the distributor. The first liquid which comes off falls between the sides of the vessel and those of the float; the latter is soon raised, and its aperture ceases to correspond with that of the valve seat. The liquid continuing to arrive, its level overtops the edge of the float, which from this instant fills more and more. When it is half full or nearly, its weight exceeds that of the liquid which it displaces; it then descends, and at the moment it comes to a position of rest, its aperture corresponds with that of the valve seat, and the liquid escapes outwards. From that it lightens itself more and more. If the exit connector of the liquifier does not compensate the loss which it produces, it will continue to ascend, and to stop the distribution, which it is its business to make to the refrigerator. But as will be seen, this suspension can only take place when it becomes necessary, that is to say, when the reserve liquid is nearly exhausted.

To the exit aperture of the distributor is fitted a tube of small diameter, of any length or degree of curvature, which ultimately carries into the interior of the refrigerator the liquid produced by the cold, and which thus terminates the first part of the circuit. This tube, before entering the refrigerator, is furnished with a stop-cock, which is the first that is met with from the time of leaving the boiler, or the starting point of the circuit. To show how necessary this arresting point is, it will suffice to remark, that the tension of the vapor in the refrigerator should be about one atmosphere, or perhaps a little more, as we shall see immediately, while it is eight atmospheres in the boiler.

With this excess of seven atmospheres, then, the vapor in the boiler would rush violently into the refrigerator, if this first stop-cock were not interposed. It is, then, indispensable. It is this which controls the effect of this excess of pressure, which stops it off at will, and which reduces it to what it ought to be, in order that the liquid may be injected into the refrigerator at a suitable rate.

It would be superfluous to describe the refrigerator here, because its form and dimensions depend on the effects which it is wished to produce. They will be very different, whether it be intended to make ice, or to cool masses of liquid, which are changed with more or less quickness. We shall confine ourselves to saying that the form of the refrigerator is far from arbitrary, and that in every case there are two essential conditions to which it remains subject, namely, to expose large evaporating surfaces always wetted with liquid, and at the same time, to give a very free circulation to the vapor, and also to collect in a small space the residues of the evaporation, which become more and more hydrated, and from which it is necessary to free the refrigerator from time to time, by certain and easy means.

(To be Continued.)

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, No. 202.

(Continued from page 30.)

2. *To the Death of Galileo.*—Science made but little progress during the middle ages, but that such was the case is not to be wondered at, when we consider the constitution of society at the time.

The chief elements in that society were the feudal barons and the priests. The barons were perpetually at war with their neighbors, war was their profession, and study of any kind was distasteful to them; their minds were never required to think deeply, and soon lost the power of doing so for want of practice. The priests were often men of learning, but they devoted their energies solely to theology, for their church considered the scientific dogmas of Aristotle sufficient for all purposes, and forbade speculations in reference to the causes of natural phenomena. It was the church therefore which raised obstructions to prevent the further progress of science, it was the church that did not allow men to think for themselves, and which undoubtedly by so doing retarded the civilization of Europe for centuries. We have not yet recovered from the effects of that edict.

There were, no doubt, great and original scientific thinkers during the middle ages, but their thoughts too often perished with them, for they did not dare to oppose Aristotle openly, and but little thought must have shown them the fallacy of his reasoning.

The alchemists in their search for the philosopher's stone collected together a great mass of matter, out of which has been extracted a few facts of practical utility, for labor applied to the elucidation of the most visionary and impossible theory is never entirely lost; purged of its dross, a little globule of pure metal always remains. I believe if a man were to try to harness a comet to a railway train, although ninety-nine parts of his labor would be lost, the hundredth would be of profit to the world.

In the writings of the alchemists we usually find a chapter devoted to the four elements, and the conversion of one into the other, according to Aristotle's theory, is generally represented by a diagram. The following is taken from a work on Alchemy* published in 1546:—

Fire is represented by an angel, air by a bird, water by a dragon, and earth by a bull.

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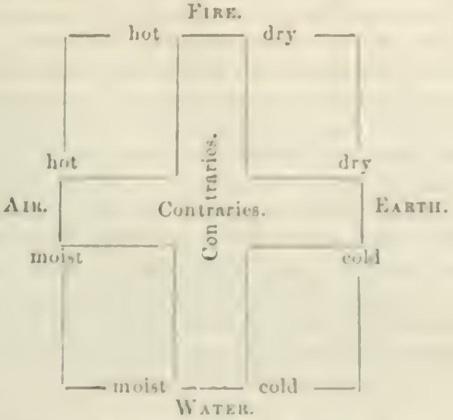
* "Pretiosa Margarita Novella de Thesauro," &c., 1546.

The manifestation of a taste for scientific studies was very apparent in Italy in the middle of the sixteenth century, and several scientific societies were then founded.



Giovanni Baptista Porta* was one of the greatest cultivators of science of this period; he was the author of numerous works on a variety of subjects; among them I may mention several works on physiognomy, several comedies, and treatises on "The Art of Remembering," "On Distillation," "On Refraction," "On Natural Magic," and "On the Transmutations of the Air;" the two last alone claim our attention.

The treatise on "Natural Magic" is Porta's best known work. In the preface to the edition which I have consulted, Porta informs his readers that he first published the work when he was scarcely fifteen years old, and that it was well received and translated into Italian, French, Spanish, and Arabic; he now sends it forth again to the world thirty-five years later. The contents were taken both from the writings of the ancients and from those of his contemporaries; libraries in France Italy, and Spain were ransacked; Porta's own personal observations in various countries were added, and he was assisted by "nobles, potentates, great and learned men;" moreover, money was willingly contributed to pay for the expenses of trying experiments.

Let us leave the preface, and look at the result of all this labor.

The Treatise is divided into 20 Books.

Book 4 treats "Of increasing Household Stuff."

Book 7 gives a good account of the principal properties of the loadstone, and is the most philosophical treatise in the whole work.

Book 8 "Of Physical Experiments," is a very curious production. We find among the experiments—"To make a man out of his senses for a day," "To fasten the teeth," and an account of "A preservative against envy."

Book 9 treats "Of Beautifying Women."
Book 13 "Of Tempering Steel."
Book 14 "Of Cookery."
Book 20 "Of the Chaos."

* Born, 1545. Died, 1615

This work gives us some idea of the scientific literature of the period. Here was a man of undoubted literary tastes, with every possible facility given him for the compilation of his book, with thirty-five years to collect and arrange his subjects, and to build them up on a foundation already laid, with the assistance of the most learned men of the time; in a word, furnished with everything which could make his work the most scientific which had yet appeared, and when he gives it to the world he is found to have written on the most diverse and eminently unscientific subjects, which are, moreover, badly arranged and badly handled. Still this work was a step in the right direction; Porta's predecessors had for the most part written on sorcery and necromancy, and had dealt in such arts; Porta strongly repudiates the assertion that he was a sorcerer, and endeavors to show that the magic of nature is as wonderful as that of wizards and witches; and as a work written for such a purpose, we must regard the "Natural Magic," in spite of its faults, as one of the channels by which the human mind escaped from the stagnant lake of middle age ignorance and superstition.

Porta's treatise on the "Transmutations of the Air," contains a table which begins with air and ends with forty-three entities, into which he believes it can be transmuted; the following are among them:—

Air vehemently agitated by the orb of the moon, becomes fire. Rarefied by the heat of the sun—wind. Moderately condensed—Clouds. Congealed—snow.

Rarefied by heat beneath the earth, air produces earthquakes, and by other causes it becomes dew, meteors, thunder, lightning, whirlwinds, &c., &c. This work contains chapters on rain, snow, hail, clouds, and on other subjects connected with meteorology.

Our own countryman Francis Bacon,* did more to engraft in the human mind a love for science than any of his predecessors. He was the founder of a new epoch in the history of science, the advocator of a new method of addressing questions to Nature, the very father of all our experimental sciences. Not only did he give us the true method of conducting scientific inquiry, but he also put many of his own suggestions to the test by actual experiment; a few of his experiments relate to the air.

Bacon believed that air was produced from water, for he tells us that a drop of water turned into air occupies a hundred times its former bulk; and he considers it very probable that air may be condensed to water by subjecting it for some months to a freezing temperature, because we see that solid bodies are permanently affected by being kept in a state of strain; a stick if kept bent for a considerable time does not regain its former shape; may it not happen that the air will be permanently affected by violence?

In the forty-fifth aphorism of Book II. of the "Novum Organum," Bacon mentions an experiment which he made to discover to what extent air could be rarefied. He took a glass egg, and withdrew by suction as much air as was possible by the means; the orifice was then closed by the finger, and the egg immersed in water, which, on opening the orifice, of course rose to a certain height in the egg, the reason of which he explains thus :*—"The air being constrained by the effort made in suction, and dilated beyond its natural state, and therefore striving to recover and contract itself (so that if the egg had not been immersed in water it would have drawn in the air with a hissing sound) now drew in a sufficient quantity of water to recover its former dimensions." It is difficult from this explanation to understand in what light Bacon viewed the rushing in of water; he seems to have considered that suction acted on the air in the egg as force acts in bending a stick; and the contraction of the air an effect similar to that of a bent stick regaining its shape, and it was natural to compare one elastic body with another. We must bear in mind that he knew nothing of the pressure of the air, for he considered it imponderable.

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* Born, 1561. Died, 1626.

It would be a great advantage, he writes, if bodies could be endued with fixed properties "by violence," and he recommends it to be tried, if, when a glass egg is exhausted of air by suction, the orifice closed, and the whole put aside for some days, the air rushes in with a hissing sound on opening the aperture, and if as much water as before enters the egg.

If onions are hung up in the air they are found after a time to have sprouted. Bacon recommends that they should be weighed before and after sprouting; if they have gained weight it will be a proof that "air may be made to condense, and also that air can nourish."

There can be no doubt, writes Bacon in the "Sylva Sylvarum,"† that the airs differ in different localities, because it is well known that "confitures and pies" become mouldy, and meat rots in some airs much sooner than in others. The tests which he gives for the purity of air are curious. Air may be considered wholesome if wool hung up in it does not increase much in weight, if meat does not rot quickly, and if the weather-glass undergoes no sudden change. The weather-glass here mentioned had been but recently invented; it was the instrument which we now call the air thermometer, and therefore showed changes of temperature as well as changes of the air's pressure. There has been much dispute as to who invented this thermometer; some attribute it to Santorio, of Padua, others to Cornelius Drebbel, but the generally received opinion seems to be that it was invented by Galileo in 1597.

Galileo‡ was the contemporary of Bacon, and we occasionally find him quoted in the "Novum Organum."

He entered the field of science at a time when, from neglect, it was overgrown with all manner of rank and stunted forms, and required a strong hand to chop and cut right and left to remove the weeds, and to plant new seed. Galileo attacked the dogmas of Aristotle, and proved their fallacy, but with the very proofs before their eyes, men refused to admit the new doctrines, and it was not till long after the publication of the celebrated "Mathematical Dialogues," that Aristotle's physical theories received their death-blow.

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* According to J. Devey's translation.

† Published after the author's death by W. Rawley. Tenth Edition, 1676, consulted.

‡ Born, 1564. Died, 1642.

In the "Mathematical Dialogues"* several experiments relative to the air are mentioned.

Nothing, he says in the first Dialogue, shows us Nature's abhorrence of a vacuum better than the experiment in which two plates of metal or marble, ground with very smooth surfaces, when pressed together, are found to adhere with such force that we can lift one by the other. In order to show "the power of a vacuum," Galileo took a hollow cylinder of metal, smoothly turned inside, and closed at one end; into this he fitted accurately a short solid cylinder of wood, to which a hook for supporting weights was attached. The metal cylinder was filled with water, and the wood pressed down upon its surface; on now inverting the vessel he found that a considerable weight might be attached to the hook without separating the wood from the water, the surface of the wood was connected with the water according to Galileo, by no other cause "but the repugnance to a vacuum," and if we know the weight of the wood, the hook, and the added weights, we obtain the "quantity of the force of the vacuum."

It is curious that a philosopher with these facts before his eyes could have failed to discover the pressure of the air; but we arrive now at the observation which ultimately led to the discovery of that great fact.

On one occasion Galileo observed a cistern, to draw water out of which was a pump; "this pump," he writes, "had its sucker and valve on high, so that the water was raised by attraction and not by impulse as it is in those pumps which work below. This drew the water in great plenty whilst the water was at a certain determinate height in the cistern, below which, if the water subsided, the pump was useless. The first time I observed this I thought the engine had been broke. I sent for a workman to mend it, who told me that this defect was owing to nothing but to the waters being too low, and thence not suffering itself to be raised to such a height; and, he added, that neither pump nor any other machine which raises water by attraction could ever raise the water a hair's breadth more than eighteen cubits; and, let the pump be big or little, that this is the utmost height. Now, although I knew that a rope, a wooden staff, and an iron rod might be so lengthened that, lifting them upright, they would break by their own weight, yet I was so silly as never till now to consider that the same thing would much more easily happen in a string or thread of water; and what else is that which is attracted within the pump but a cylinder of water, which, being more and more prolonged, at length arrives at that term beyond which, if it be elevated, it, like a rope, is broke by its own excessive weight?" Such was Galileo's explanation of the phenomenon which led to Torricelli's discovery of the pressure of the air.

Galileo believed that the particles of matter are held together by having a vacuum between them, penetrable by fire but not by air.

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* First published in 1632; done into English by T. Salisbury in 1661, and by Thomas Weston in 1730. Weston's translation has been consulted.

Galileo found that air retarded the velocity of a projectile, and he proved the "difference of velocities in movables of different gravities to be more and more as the mediums are more and more resisting;" and hence, he continues, "I have thought that if the resistance of the mediums be wholly taken away, all matter would descend with equal velocity."

Galileo was the first to prove satisfactorily that the air possessed weight. In order to show this he took a glass bottle with a narrow neck, fitted it with a leather cover and valve, and by means of a syringe compressed a quantity of air within it; the bottle and compressed air were now carefully weighed, the weight being adjusted by fine sand; he then opened the valve, and thus let out the compressed air. On reweighing, the bottle was found to be lighter, and a quantity of sand had to be removed from the other scale. There can be no doubt, he says, that the sand removed represents the weight of air which was violently forced into the bottle.

Galileo made use of two methods to determine the specific gravity of the air. By the first compressed air is caused to form water from a vessel; by the second a vessel is taken, and a quantity of water forced into it without allowing any air to escape. The air thus suffers compression; the whole is now weighed, the valve opened, and the compressed air allowed to escape. The air which escapes is obviously equal in bulk to the water in the bottle, and, by again weighing, the weight of air which has escaped is found; the water is also weighed, and thus we arrive at the relative weights of the two.

(To be Continued.)

French Cement.

From the London Chemical News, No. 231.

This cement composed of lime and india-rubber, is very valuable for mounting large microscopical preparations. The principle advantages are—that it never becomes perfectly hard, and thus permits considerable alteration to take place in the fluid contained in the cell without the entrance of air, and it adheres very intimately to glass, even if it be perfectly smooth and unground. If a glass cover is to be affixed to a large cell containing fluid, a small piece of the cement is taken between the finger and thumb and carefully rolled round until it can be drawn out into a thread about the eighth or tenth of an inch in thickness; this is applied to the top of the cell, before introducing any fluid and slightly pressed down with the finger previously moistened. It adheres intimately. The preservative fluid with the preparation are now introduced, and the cell filled with fluid, which indeed is allowed to rise up slightly above the walls. The glass cover, rather smaller than the external dimensions of the cell, and slightly roughened at the edges, is to be gently breathed upon, and then one edge is applied to the cement, so that it may be allowed to fall gradually upon the surface of the fluid until it completely covers the cell and a certain quantity of the superfluous liquid is pressed out. By the aid of any pointed instrument a very little cement is removed from one part, so that more fluid may escape as the cover is pressed down gently into the cement. The pressure must be removed very gradually, or air will enter through the hole. A bubble of air entering in this manner may often be expelled again by pressure, or it may be driven out by forcing in more fluid through a very fine syringe at another part of the cell, but it is far better to prevent the entrance of air in the first instance. The edge of the glass cover being thoroughly embedded in the cement, the small hole is to be carefully plugged up by a small piece of cement, and the cell allowed to stand perfectly still for a short time, when it may be very gently wiped with a soft cloth. The edges of the cement may be smoothed by the application of a warm iron wire, and any superabundance removed with a sharp knife. A little Brunswick black or other liquid cement may be applied to the edges for the purpose of giving the whole a neater appearance. The cement is made as follows:—A certain quantity of india-rubber scraps is carefully melted over a clear fire in a covered iron pot. When the mass is quite fluid, finely powdered lime, having been slacked by exposure to the air, is to be added by small quantities at a time, the mixture being well stirred. When moderately thick, it is removed from the fire and well beaten in a mortar, and moulded in the hands until of the consistence of putty. It may be colored by the addition of vermilion or other coloring matter. This cement answers well for fixing on the glass tops of large preparation jars, but if moderately strong spirit be used a little air must be permitted to remain in the jar.—*From "How to work with the Microscope," by Lionel S. Beale.*

On the Pressure of Steam at High Temperatures.

By R. A. Peacock, C.E.

From the London Artizan, Jan. 1861.

Some years ago the writer had occasion to attempt to calculate the probable pressure of steam at the highest known temperatures and found, amongst other things, that between the pressure of 25 lbs. per square inch and 300 lbs. to the square inch, the latter being the highest pressure to which reliable experiments had been carried, the law of increase was approximately as follows:—

1. The *temperature* of high pressure steam of, say, 25 lbs. to the square inch and upwards, increases as the $4\frac{1}{2}$ *root* of the pressure.
2. Conversely the *pressure* of steam of, say, 25 lbs. to the square inch and upwards, increases as the $4\frac{1}{2}$ *power* of the temperature.
3. At lower pressures than about 25 lbs. per square inch, a different law prevails.

It may, perhaps, be of interest to some to see the calculations by this formula placed side by side with the results of some of the best known experiments and formulæ. And on a future occasion the working formula, which is very simple and short, will also be given.

The following table gives a copy of the "more trustworthy of Arago and Dulong's experiments after all necessary corrections," copied from "A Treatise on Heat" by the Rev. R. V. Dixon, A.M., Dublin, 1849,

p. 173, and copied by him from "Ann. de Chim. et de Phys." tome xliii, p. 108, reduced, however, from Cent. degrees to Fahr., and from elastic force in atmosphere of 0·76″ to pressure in pounds per square inch by the present writer, an atmosphere being taken at 14·7 lbs. It will be seen that the formula does not agree with MM. Arago and Dulong's experiments, but that it does agree nearly with other well-known experiments specified afterwards.

The American Commissioners' experiments, which are not now given, differ as much from the formula as MM. Arago and Dulong's, but in a different way.

MM. Arago and Dulong's Experiments.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
31·458	254·66	252·83	+1·83
42·196	271·94	269·88	2·06
67·23	301·46	299·31	2·15
95·516	326·12	323·61	2·51
108·4198	335·30	332·84	2·46
170·99	371·30	368·31	2·99
252·619	404·24	401·68	2·56
254·089	405·32	402·20	3·12
272·008	410·90	408·33	2·57
316·858	425·12	422·42	2·70
351·8298	435·47	432·36	3·11

The following are the experiments of Dr. Wm. Fairbairn, F.R.S., given at p. 313, of "Useful Information for Engineers," second series. The formula, it will be seen, never differs as much as half a degree Fahr. from experiment; and the calculations being sometimes less and sometimes greater than the corresponding experiments, indicate that the experiments are averaged by the formula, and some of the differences are quite insignificant.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
26·5	242·90	243·37	—·47
27·4	244·82	245·19	—·37
27·6	245·22	245·59	—·37
33·1	255·50	255·71	—·21
37·8	263·14	263·36	—·22
40·3	267·21	267·14	+·07
41·7	269·20	269·17	+·03
45·7	274·76	274·70	+·06
49·4	279·42	279·50	—·08
51·7	282·58	282·34	+·24
55·9	287·25	287·28	—·03
56·7	288·25	288·20	+·05
60·6	292·53	292·48	+·05

The following are M. Regnault's experiments from 23 lbs. to the square inch up to 300 lbs. to the square inch, from which the formula never differs as much as three-quarters of a degree Fahr., and at 300 lbs. pressure it very nearly coincides with experiment.

Regnault's experiments, as now given, are copied from Dr. W. Fairbairn's "Mills and Millwork," vol. i, p. 202, obtained from the tables of M. Regnault, by interpolation and reduction to English measures.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.	Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in	Deg.	Deg.		lbs. per sq. in.	Deg.	Deg.	
23	235·43	235·83	—·40	59	291·42	290·75	+ 67
24	237.75	238·07	—·32	60	292·51	291·84	+·67
25	240·	240·24	—·24	65	297·77	297·08	+·69
26	242·16	242·34	—·18	70	302·71	302·01	+·70
27	244·16	244·39	—·13	75	307·38	306·67	+·71
28	246·32	246·37	—·05	80	311·83	311·10	+·73
29	248·30	248·30	equal	85	316·	315·32	+·68
30	250·23	250·17	+·06	90	320·03	319·35	+·68
31	252·09	252·01	+·08	95	323·87	323·22	+·65
32	253·94	253·81	+.13	100	327·56	326·92	+·64
33	255·70	255·53	+·17	105	331·10	330·49	+·61
34	257·47	257·23	+·24	110	334·51	333·92	+·59
35	259·15	258·90	+·25	115	337·84	337·24	+·60
36	260·83	260·52	+·31	120	340 99	340·44	+·55
37	262·44	262·11	+·33	125	344·06	343·54	+ 52
38	264·04	263·67	+·37	130	347·05	346·55	+·50
39	265·58	265·20	+·38	135	349·93	349·47	+·46
40	267·12	266·70	+·42	140	352·76	352·30	+·46
41	268·60	268·16	+·44	145	355·6	355·06	+·54
42	270·07	269·60	+·47	150	358·3	357·75	+·55
43	271·50	271·02	+·48	160	363·4	362·91	+·49
44	272·91	272·40	+·51	170	368·2	367·84	+·36
45	274·30	273·77	+·53	180	372·9	372·55	+·36
46	275·65	275·11	+·54	190	377·5	377·04	+·46
47	276·99	276·42	+·57	200	381·8	381·37	+·43
48	278·30	277·72	+·58	210	386·	385·52	+·48
49	279·59	279·	+·59	220	389·9	389·52	+·38
50	280·85	280·25	+·60	230	393·8	393·39	+·41
51	282·09	281·49	+·60	240	397·5	397·13	+·37
52	283·32	282·70	+·62	250	401·1	400·75	+·35
53	284·53	283·90	+·63	260	404·5	404·26	+·24
54	285·73	285·08	+·65	270	407·9	407·07	+·23
55	286·90	286·25	+·65	280	411·2	410·97	+·23
56	288·05	287·40	+·65	290	414·4	414·19	+·21
57	289 19	288·53	+·66	300	417·5	417·32	+·18
58	290·31	289·65	+·66				

It will be observed that the discrepancies between the formula and M. Regnault's table are not confirmed by Dr. Fairbairn's experiments. For example, with the pressure 56 lbs. in Regnault's table, we have a difference between that table and the formula of +·65, whereas between Fairbairn's table for the pressure 56·7 lbs. and the formula, we have a difference of only +·05, and the like is observable with respect to Fairbairn's pressure of 60·6 lbs., and Regnault's pressure of 60 lbs. so that it is probable the apparent discrepancy which gradually increases up to +·73 and then diminishes again between the formula and Regnault's table, does not really exist.

The following table is given in *The Artizan* for October, 1863, p. 219, the third and fourth columns being calculated by the present writer as before.

Pressure.	Mr. Birckel's Temperature.	Temperature Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
24·54	239	239·255	—·255
28·83	248	247·976	+·024
33·71	257	256·745	—·205
39·25	266	265·574	+·426
45·49	275	274·426	+·574
52·52	284	283·33	+·67
60·40	293	292·27	+·73
69·21	302	301·25	+·75
79·03	311	310·264	+·736
89·86	320	319·24	+·76
101·90	329	328·285	+·715
115·10	338	337·3	+·7
129·80	347	346·43	+·57
145·80	356	355·5	+·5
163·30	365	364·564	+·436
182·40	374	373·63	+·37
203·3	383	382·75	+·25
225·9	392	391·82	+·18

This table, by Mr. J. J. Birckel, agrees very nearly with M. Regnault's.

In the November number of *The Artizan*, Mr. Macquorn Rankine, F.R.S., publishes a paper "On the Expansive Energy of Heated Water," in which he gives a table, which we quote from; and which, in its first part, consists of some of the identical figures just given under the name of Mr. J. J. Birckel.

Initial absolute Pressure.	Initial Temperature Fahrenheit.	Initial Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
28·83	248	247·976	+·024
52·52	284	283·33	+·67
89·86	320	319·24	+·76
145·8	356	355·5	+·5
225·9	392	391·82	+·18
336·3	428	428·05	—·05
	about 2360	729,632 lbs. or 325·7 tons.	

The foregoing is Mr. Macquorn Rankine's table—the last two columns being added in the usual manner by the present writer.

(To be Continued.)

Artificial Light and Lighting Materials. By B. H. PAUL, Esq.

From the Journal of the Society of Arts, No. 593.

The use of artificial light being one of those appliances which belong to an advanced state of civilization, it has naturally undergone that kind of progressive development which is characteristic of such arts. There has been, within the last 100 years, not only an immense improvement in the methods of obtaining artificial light, but also an equally important change in the sources whence lighting materials have been derived.

Towards the end of the last century the only materials used for lighting were animal fats, such as tallow and fish oils, the former being used as candles, the latter burnt in lamps. Improvements were, from time to time, made in the preparation of these materials, by the introduction of new substances, and also in methods of using them; but those improvements, though highly advantageous, sink into insignificance when compared with the introduction of coal gas as a lighting material. In towns, the use of gas soon prevailed over the use of other lighting materials, but in the country, and especially in remote places, candles and lamps still continue to be largely used. The advantages of gas are indeed of such a nature as to require a thickly populated town or district in order to be fully realized.

One of the materials formerly used for lighting, to a small extent and in a few localities, was petroleum; and some years before the introduction of gas, attempts were made to prepare artificial petroleum from various bituminous minerals, in order to use it as a source of light. These attempts were progressing fairly, when the discovery that the inflammable gas obtainable from coal by heat, could be used for lighting purposes, was made known by Murdoch, and this new method of lighting became, for a time, almost the sole object of attention in regard to artificial light. The manufacture of artificial petroleum was, however, continued to some extent, in a few places, where circumstances were unfavorable for the introduction of gas-lighting. About the year 1830 works were established in France for this purpose, and have continued in operation to this day. Somewhat later, other works were started in other parts of the continent as well as in this country; about the year 1853, the earth oil of Rangoon was examined by Mr. Warren De La Rue and Hugo Müller, with a view to its application for lighting purposes, and about the year 1860, the extraordinarily abundant supplies of petroleum in America became the object of special interest as materials for lighting.

Chemical Nature of Hydro-carbon Oils used for Lighting.—It is mainly to the products of natural and artificial materials, known under the various names of photogen, paraffin oil, &c., that I now beg to request your attention. Whether these materials be of natural or artificial origin, they all appear to agree in possessing the same general character, and in being of the same chemical nature. The chemical history of these materials is still very incomplete; but according to the results of the best chemical investigations, they appear to consist, for the most part, of mixtures of a considerable number of hydro-carbons, which differ from each other by small degrees in physical character and chemical compositions, but at the same time present a general analogy among each other, and with the gaseous hydro-carbon known as marsh gas. This substance constitutes the initial and typical member of the series, while the other extremity of the series, are solid hydro-carbons, resembling wax or spermaceti in appearance, constituting a number of substances known to chemists under the collective name of paraffin. Between these two extremes there are a great number of other substances, solid, liquid and gaseous, some of which have been separated and examined; others have not yet been obtained in an isolated state.

In addition to the hydro-carbons above enumerated, some others, belonging to other chemical series, have been detected, in smaller and varying proportions, both in the various kinds of natural petroleum, and in the tarry oils obtained by distilling bituminous minerals, peat, wood, &c., at a temperature not exceeding dull redness. These latter also contain a considerable proportion of oxygenated oils, which partake in general of the nature of creosote. In the natural petroleum the amount of these oxygenated oils is very much smaller, and it is only in some few instances that their presence has been recognised with any degree of certainty. Both in the natural petroleum and in the oils obtained by destructive distillation, there is always more or less of a kind of pitchy substance in solution, which gives to these liquids their characteristic black and tarry appearance, and there are likewise small quantities of strong smelling substances, to which the peculiar and generally offensive smell of these materials is due.

Origin of Natural Hydro-Carbon Materials.—The very intimate chemical relation between native petroleum and the oily products of dry distillation, is highly interesting from the probability which it appears to suggest, that these materials, whether natural or artificial, have a common, or at least, a closely analogous origin. The close resemblance between petroleum and the oily products of dry distillation has long been recognised, and the actual identity of a great many of their several constituents which has been only recently ascertained, is but a more detailed elucidation of the obvious analogy between these materials. Guided by this analogy, many naturalists have come to the conclusion that petroleum, in the various states in which it is met with, is the product of a kind of natural distillation that has taken place, or may be still going on, in the interior of the earth.

Whether or not this speculation be strictly correct, it undoubtedly presents a high degree of probability. All organic substances which are not themselves volatile, such as wood, flesh, and other vegetable and animal substances, yield when subjected to the influence of heat below dull redness, tarry oils, which have in all cases the same general character and similarity to petroleum; differing only according to the specific differences in the materials from which they may have been obtained.

The various kind of bituminous minerals met with chiefly in the coal measures, and to some extent in other geological situations, also yield, under similar conditions, oily products of the same general character. This fact at once becomes intelligible when it is remembered that the bituminous portion of the minerals from which such products are obtainable, has originated from the same kind of vegetable and animal substances that are now met with in nature. Hence, whether it be supposed that natural petroleum has been produced by an actual distillation of bituminous minerals, or by some other process, different, and slower in its operation, though effecting the same result as distillation, it is not the less evident that the ultimate sources of the minerals which serve for the manufacture of those lighting materials to which I am now referring, are in all cases the same—namely, organic substances, and those, in all probability, for the most part of vegetable origin.

There is, however, an important difference between these bituminous minerals and unaltered organic substances, in regard to their fitness for yielding by distillation such products as are suitable for the manufacture of hydro-carbon lighting materials. This is the difference that exists between the composition of unaltered organic substances and the composition of those remains of organic substances belonging to former epochs, which constitute the bituminous portion of the minerals to whose formation they have partly contributed.

Taking, for the sake of illustration, dry wood as representing the composition of the organic substances, which are the primary sources of bituminous minerals, and comparing it with the average composition of the bituminous portion of coal, it appears that for a given per centage of hydrogen the relative amounts of carbon and oxygen are widely different.

	Wood.	Coal.
Carbon,	50·00	88·15
Hydrogen,	5·55	5·26
Oxygen,	44·45	6·59
	100·00	100·00

This difference arises from the alteration the organic substances have undergone during their passage into that bituminous portion of minerals by which they are now represented. This alteration consists in a progressive elimination of the oxygen, either as water or carbonic acid or both, and the consequent concentration of carbon, as may be seen from the following representation of this change:—

	Composition of wood.		Composition of water.		Residue.		Composition of carbonic acid.		Residue.
Carbon,	50·00				50·00	—	6	=	44·00
Hydrogen,	5·55	—	1	=	4·55				4·55
Oxygen,	44·45	—	8	=	36·45	—	16	=	20·45
	100·00	—	9	=	91·00	—	22	=	69·00

This change is analogons to that which takes place in decaying organic substances, and is, in fact, a slow carbonization. Its effect is of considerable importance as regards the artificial production of hydro-carbons for lighting purposes, inasmuch as the value of any material for this purpose increases with the increase in the amount of hydrogen, and the decrease in the amount of oxygen they contain. It is on account of the large amount of oxygen in wood and peat, as compared with coal, that those materials are less suitable for the manufacture either of illuminating gas or hydro-carbon oils. In the former case the gas produced from them contains such a large amount of carbonic acid, that its illuminating power is very slight, and in the latter case, the oils produced by distillation, are contaminated with oxygenated oils and resinous substances, which render their purification more troublesome than that of the oils obtainable from bituminous minerals, in which the amount of oxygen remaining is so much less.

But there is another feature in this alteration of the organic substances from which the bituminous portion of the minerals in question has originated. By referring to the above comparison between the composition of wood and that of coal, it will be seen that not only has the oxygen been, to a great extent, eliminated, but there has been an alteration in the ratio existing between the hydrogen and carbon. Instead of being, as in the case of wood, in the ratio of 1 hydrogen to 9 carbon, it is in coal in the ratio of 1 hydrogen to 17 carbon. This circumstance is indicative of the separation, during the change that has taken place in the conversion of decaying organic remains into the constituent bituminous minerals, of a compound of carbon and hydrogen, such, for instance, as marsh gas.

This gas, consisting of carbon and hydrogen in the ratio of 3 to 1, is always produced in putrefaction, and by the action of heat upon organic substances. It also occurs naturally in connexion with the coal measures, constituting, together with small quantities of olefiant gas and nitrogen, the main portion of the fire-damp of coal mines. Carbonic acid gas is also of frequent occurrence in the coal mines. These facts, together with the close chemical relationship between marsh gas and the various constituents of petroleum, all agree in adding to the probability of a very close connexion between bituminous materials and the various kinds of petroleum; a connexion of such a nature, in all probability, that they should be regarded as a simultaneous origin, in the same way that the carburetted hydrogen or marsh gas of coal mines, has most likely originated, not from the decomposition of coal itself, but conjointly with coal, by the progressive alteration of the organic remains whence it and other bituminous minerals have been formed.

Exhalations of marsh gas almost always accompany the discharge of petroleum from natural springs, and in most of the American petroleum it is possible to recognise the presence of small portions of the other substances, ranging next to marsh gas in the above series, and gaseous under ordinary conditions of temperature and pressure.

	Carbon.	Hydrogen.	Sp. Gr.	Carbon in one gallon.	Boiling point, Fahr.	Vapor density.	Carbon in equal volumes of vapor.	Volume of air requisite for burning equal volumes of vapor.
				lbs.				
1 Methyl-hydride $C_2 H_4$ (marsh gas).	75·00	25·00	—	—	gas	0·554	—	10
2 Ethyl-hydride $C_4 H_6$ . .	80·00	20·00	—	—	"	1·04	—	—
3 Propyl-hydride $C_6 H_8$ . .	81·81	18·19	—	—	"	1·52	—	—
4 Butyl-hydride $C_8 H_{10}$ . .	82·76	17·24	—	—	"	2·01	—	—
5 Amyl-hydride $C_{10} H_{12}$ . .	83·33	16·67	0·628	5·233	86	2·49	2·5	—
6 Hexyl-hydride $C_{12} H_{14}$ . .	83·72	16·28	0·669	—	158	2·97	3·0	—
7 Heptyl-hydride $C_{14} H_{16}$ . .	84·00	16·00	0·699	5·9	198	3·46	3·5	70
8 Octyl-hydride $C_{16} H_{18}$ . .	84·21	15·79	0·726	—	243	3·94	4·0	—
9 Nonyl-hydride $C_{18} H_{20}$ . .	—	—	0·747	—	278	—	4·5	—
10 " $C_{20} H_{22}$ . .	84·50	15·50	0·757	—	321	—	5·0	—
11 " $C_{22} H_{24}$ . .	84·61	15·39	0·766	—	359	—	5·5	—
12 " $C_{24} H_{26}$ . .	—	—	0·776	—	408	—	6·0	—
13 " $C_{26} H_{28}$ . .	84·78	15·22	0·792	—	423	—	6·5	—
14 " $C_{28} H_{30}$ . .	84·84	15·16	0·800	6·787	460	6·85	7·0	—
15 " $C_{30} H_{32}$ . .	—	—	—	—	496	—	7·5	—
16 Cetyl-hydride $C_{32} H_{34}$ . .	—	—	—	—	527	7·825	8·0	—
17 " $C_{34} H_{36}$ . .	85·00	15·00	0·825	7·022	. .	8·37	8·5	—
18 " $C_{36} H_{38}$ . .	—	—	—	—	. .	—	9·0	—
19 " $C_{38} H_{40}$ . .	—	—	—	—	. .	—	9·5	—
20 " $C_{40} H_{42}$ . .	85·10	14·90	—	—	. .	—	10·0	—
21 " $C_{42} H_{44}$ . .	—	—	—	—	. .	—	10·5	—
					Melting point.			—
25 Candle paraffin? $C_{50} H_{52}$ . .	85·23	14·77	0·870	7·41	130	12·19	12·5	—
27 Ceryl-hydride $C_{54} H_{56}$ . .	—	—	—	—	136	—	13·5	—
30 Myricyl-hydride $C_{60} H_{62}$ . .	85·31	14·69	0·890	—	143	14·61	15·0	—
Olefiant gas $C_4 H_4$ . .	85·71	14·29	—	—	gas.	0·97	1·0	15

Characters of Hydro-Carbon Lamp Oils.—In the absence of any important difference between those constituents of natural petroleum and of the oils obtained by distilling bituminous minerals, which are capable of being employed as lighting materials, it will be unnecessary to draw any special distinction between the oils manufactured for that purpose from different materials; and, in considering the characters which are requisite in these products, they need be contrasted only in regard to certain minor details, that are more consequences of the methods of manufacture practised, than of any more essential difference.

By examining the tabular statement of the composition and characters of the various substances constituting both petroleum and the so-called coal or shale oils, and considering what are the conditions under which the oils prepared from these materials are to be used for lighting purposes, it will be evident that there is only a portion either of the natural or artificial material that is applicable as oil for burning in lamps.

Leaving out of consideration the first four members of the series, which are gaseous at ordinary temperatures, it will be seen that the remaining members of the series, which are liquid at ordinary temperatures, gradually increase in specific gravity, varying in this respect to the extent of 6¼ pounds per gallon to about 8¾ pounds per gallon. Beyond this latter point the other members of the series are solids, and come under the general denomination of paraffin.

Another important point of difference consists in the boiling point of the individual members of the series. This varies from 87° Fahr. to upwards of 600° Fahr. It is to this character that attention requires to be chiefly directed in regard to the degree of safety with which these substances can be used for burning in lamps. I say the *degree* of safety, because I am far from being disposed to agree with the opinion that has been put forward that the use of any of these substances is necessarily attended with danger. On the contrary, I am strongly inclined to think that all or any of them may be used without any necessary risk being incurred. Of course, due precautions must be observed in the use of these substances, and those precautions must be greater than are requisite in the case of the oils that were formerly used as lighting materials, viz: the fat oils obtained from animals, fish, and plants. Hydro-carbon oils are, by their nature, much more combustible than fat oils, at the same time that they possess higher illuminating power, and it would be by no means prudent to deal with the one in the same manner as with the other.

The outcry that has been raised against the dangerous character of hydro-carbon oils is, however, in no degree more deserving of respect than that which was raised on the same visionary ground when the use of gas was first introduced. All that is really necessary to avoid accident with the hydro-carbon oils is, that those who use them should be properly instructed as to the way in which they should be stored and used. By this means it will be far more likely that the desired result of preventing accident may be secured, than by exciting a vague and unreasonable apprehension of danger.

In regard to the safety of hydro-carbon oil used for burning in lamps, the main consideration is its capability of giving off vapor spontaneously at the ordinary temperature, or when the oil becomes slightly warmed. In regard to this character the various liquid members of the substances given in the table differ considerably. Taking the first of those that are liquid at the ordinary temperature, it will be seen that its boiling point, which is the thermometric indication of its degree of volatility, is 86° Fahr., a temperature so little above that which may exist in a room where lamps are used, that it would be very objectionable to use such a liquid as a lighting material in that way. The temperatures given in the table as the boiling points of the individual oils are not by any means the lowest temperatures at which they evolve vapor, and for this reason, if we fix 80° or 90° Fahr. as the highest temperature to which the oil in a lamp is capable of being raised, even in an extreme case, the boiling point of a liquid that would be suitable for burning in lamps, under such conditions, ought not to be less than 200° Fahr. above that temperature.

With the lamps now generally in use for burning hydro-carbon oils, it would be only under very exceptional and improbable circumstances that the oil in the lamp could become heated to a temperature so high as 80° Fahr., and even then some of the more volatile portions of these oils, boiling at 160° Fahr., might be burnt without any danger.

But the hydro-carbon lamp oils are never distinct substances in a chemical sense, like the several substances whose characters are described in the above table. They are always mere mixtures of a number of those substances, and consequently they do not present any constant point of ebullition, while the specific gravity and other characters are the mean of those appertaining to the several substances present in the mixture, and that mean also varies according to the relative proportions of the mixed substances.

The absolute separation of these substances is, indeed, a matter almost of impossibility, and, so far as relates to their practical application, not by any means requisite. It is, however, essential that in preparing hydro-carbon oil for use in lamps, the manufacture should be conducted in such a manner as to secure the separation of the more volatile portions, to such an extent, that the oil, when finished, may be heated to 100° Fahr. without taking fire when the flame of a match is brought into contact with its surface in an open vessel such as a saucer. Oil that will bear this test is, I believe, perfectly safe for all reasonable use; and I am of opinion that this simple test is by far the best criterion of the proper character of such oils. While, of the oils constituting petroleum and coal oil, those which have a boiling point below 280° Fahr. would be too volatile for ordinary use in lamps; on the other hand, those which have a boiling point above 600° Fahr. are also unfit for this purpose, but for very different reasons. It has commonly been supposed that the reason why these latter oils do not burn well in the ordinary lamps is, that they are too thick, and have not sufficient capillarity to rise in the wick and feed the flame. I am disposed to think that this view is erroneous, and am more inclined to the opinion that their defects, as lamp-oils, are to be ascribed rather to the high temperature requisite for their volatilization, and also to the greater density of their vapor. The chief difficulty that is experienced in the use of these oils of high boiling point, which are generally known as heavy oils, is the charring of the lamp wicks, and the small flame they give.

By reference to the table it will be seen that the oil which has a boiling point of 527° Fahr., affords a vapor that is twice as dense as that of the oil boiling at 243° Fahr., or, in other words, a given weight of the former oil occupies in the state of vapor only half the bulk of an equal weight of the latter also in a state of vapor. This circumstance alone will, to a great extent, account for the smaller flame of the heavier oils as compared with that of the more volatile oils, whose vapors are less dense. Then, in proportion as the volatility of the oils decreases, or as the temperature requisite for their volatilization increases, at the same time a greater degree of heat is necessary to effect that decomposition of the vapor into gas, which is an essential step in their combustion for lighting purposes. The temperature necessary to effect these changes is so high, that at the same time the wick itself gradually becomes charred and incapable of exercising that capillary action by which the flame can be fed with oil. In addition to these circumstances, it must also be remembered that in proportion as the density of the oil vapor increases, so does the amount of carbon in the vapor increase, and, at the same time, there is a proportionate increase, both in the heating power and the illuminating power of equal volumes of the vapor. Consequently, in using heavy oil of high boiling point in a lamp, the result is that a very small, intensely hot and highly luminous flame is produced at first, and after a very short time the wick becomes charred and incrusted with the surplus carbon, separated in the flame, to such an extent that it soon begins to decrease in size and to lose brilliancy.

It appears, therefore, that the oils at either extremity of the series are equally unfit for use in the ordinary hydro-carbon oil lamps. But in practice it never happens that any one of the several substances described in the table is used for lighting purposes. It is always a mixture of a number of them that is used. Still, the facts that I have just pointed out in regard to the extreme cases, exercise their influence in the same manner with regard to this mixture. On the one hand, it must not contain any such amount of the more volatile members of the series, as will confer upon the mixture the property of giving off inflammable vapor when heated to 100° Fahr., or enable it to become permanently inflammed when brought in contact with a lighted match while at that temperature. On the other hand this mixture must not contain such a proportion of the heavier and less volatile oils, as would render the vapor forming the flame too dense and too highly carbonaceous, and necessitate too high a temperature for that decomposition of the vapor which is essential to the production of light.

The due proportion of these various conditions is secured in practice by collecting the oils during distillation in fractional portions: that which distils over first containing the oils that are too volatile for use in lamps; the second portion that distils over being the burning oil, and the third portion containing the heavy oils and paraffin.

The precise points at which this separation of the oil into these three portions, takes place, will depend very much upon the nature of the material operated upon. In this respect there are great differences in the various materials now used as sources of these lighting oils. American petroleum generally requires the separation of some 20 or 25 per cent. of its bulk, which is too volatile for use in lamps. Canadian petroleum does not require more than 10 per cent. to be separated on this account. Coal and shale oils also vary much in this respect, according to the material they are produced from, and the temperature at which the crude oil is distilled from the coal or shale. As a general rule, the crude oil of coal or shale contains a greater amount of the volatile oils, the lower the temperature those materials are distilled at. This, however, is not a disadvantage, since the amount of crude oil obtainable is greater, the lower the temperature of the distillation. Generally speaking, coal or shale oil does not require that more than 5 to 15 per cent. of the bulk should be separated as too volatile to be mixed with the burning oil.

At the other extreme there are also differences between petroleum and coal or shale oils. The burning oils manufactured from the latter sources, as generally met with, have a specific gravity of about 0·825, while the specific gravity of those made from petroleum is rarely above 0·800, and sometimes as low as 0·780.

I do not believe there is any really sufficient reason why there should be this difference in the case of petroleum, which, as I shall afterwards point out, involves a serious disadvantage as regards its illuminating power as compared with that of coal oil. The manufacture of these commodities, however, is still carried on so much more by mere rule of thumb, that under the guidance of sound principles based upon knowledge of the materials dealt with, that defects of this kind cannot be wondered at, and it will probably be some long time before these processes come to be carried on in such a way as to secure the best results both to the producer and the consumer.

(To be Continued.)

The Magnesian Light.

From the London Chemical News, No. 227.

Photographers are indebted to the perseverance of M. Sonstadt for the removal of a great obstacle. Every one knows the difficulty which has hitherto been experienced in getting a powerful actinic artificial light. Such a light is, however, furnished by the combustion of the metal magnesium, and, thanks to M. Sonstadt, this metal is now procurable at a price which makes it available for practical purposes.

Magnesium is an easily inflammable metal. A wire of considerable thickness can be ignited in the flame of a candle, and the light evolved by the combustion is of great intensity. It has been ascertained that a wire of 0·297 millimetre diameter will give as much light as twenty-four stearine candles of five to the pound. The powerfully actinic character of the light has been recently demonstrated by Mr. Brothers, of Manchester and Mr. Sydney Smith, both of whom have produced good pictures by its use.

The metal is neither ductile nor very malleable. It cannot be drawn but by employing a method devised by Dr. Matthiesen; it can be forced in a softened state through a small opening in an iron cylinder, and thus strands of wire of considerable length can be formed. The wire has been found to burn more steadily when three or four strands are twisted into a rope; and a simple clock-work arrangement will deliver such a rope to a spirit or oil lamp, in the flame of which it may be burned.

We look for important results from the use of this light. The opportunities for its use are numerous; and we may expect our collections to be soon enriched with pictures of objects hitherto shut out from photographers.

Some are talking widely of "night pictures," as though they expected, by means of magnesium, to obtain a picture of the gloomy effects of midnight on a scene. The principal use of the light will, of course, be for dark interiors; and we hope soon to see the magnificent grottoes of Adelsberg and Antiparos—which the pencil is as powerless to draw as the unaided camera to depict—revealed as brightly as the caverns in the glaciers, so well known to photographers. Another Frith may also show us the wonderful passages in the interior of the Pyramids more clearly than they have ever been seen by the traveler with the help of the two or three candles which light his way through the dark labyrinth, and enable us at our own fire-sides to gaze with awe on the vast range of subterranean tombs at Serapeum. All these and many more objects are now open to an enterprising man, who will no doubt soon be found to avail himself of them.

On the Double-cylinder Expansive Steam Engine. By Mr. Wm. Pole.

From Newton's London Journal of Arts, August, 1863.

The greatest advances made in the improvement of the steam engine, as an economical means of obtaining motive power, have resulted from the application of the principle of expansion, the advantages of which are now well known and universally appreciated among engineers. This principle has hitherto been applied to the greatest advantage in engines with a single cylinder, used for pumping purposes, as in Cornwall. In these cases, the peculiar nature of the motion admits of the steam being cut off after a small fraction of the stroke has been passed over, and allowed to expand during the remainder. When, however, the principle of expansion is applied in this mode to engines for producing rotary motion, some difficulties arise, which limit considerably the extent that the expansion may be carried to, and therefore reduce in a corresponding degree the economy attained.

The double-cylinder engine offers a mode of applying the expansive principle to a rotary motion, which removes, or, at least greatly mitigates, the objections to the single-cylinder: and it is the object of the present paper to state the nature of the advantages of the double-cylinder engine, to explain the principles on which they are based, and to show how these principles have been carried out in pactice with satisfactory results.

The original invention of the double-cylinder engine is intimately connected with the discovery and first application of the principle of expansion itself; but the unfortunate disputes which for a long time prevailed in reference to this subject have somewhat obscured the history. Having had occasion, however, some years ago, to investigate the matter, the author believes the following account represents the facts as accurately as they can be ascertained.

The double-cylinder engine was invented by Jonathan Hornblower, a mechanical engineer of considerable eminence in Cornwall, who took an important part in the application of the steam engine in that district, during the early part of Watt's career. The idea appears to have occurred to him early in the year 1776, if not before. He experimented upon a large working model, the cylinders of which were 11 and 14 inches in diameter respectively; and he published his invention in 1781, describing it as consisting in the employment of two steam cylinders,—the steam after it had acted in the first cylinder, being employed a second time in the other, by permitting it to expand itself; the two cylinders being connected together by suitable steam ports and valves. At the same time, he described also, shortly but clearly, several other inventions relating to the steam engine. One referring to surface condensing, which is so much applied in modern days; others to means of getting rid of the air and condensing water; and another invention was a steam piston, which, altered into a steam stuffing-box, is in common use at the present time. Here, therefore, was clearly developed the theoretical principle, as well as the practical application of the expansion of steam; and it is beyond dispute, that the first publication of the principle to the world was this of Hornblower's. The discovery of this principle, however, is usually ascribed to Watt, on the strength of a letter written by him to his friend Dr. Small, of Birmingham, as early as 1769,—twelve years before Hornblower published his description; in that letter Watt gives a clear and explicit description of the general principle of expansion. Notwithstanding, however, the large practice Watt had about this time, it does not appear that he ever applied expansion with any view to economy in its use till 1776, when an engine at the Soho works was altered to work expansively. In 1778, another engine, at Shadwell was experimented upon; and Watt published his invention with reference to expansion in 1782, eight months after the publication of Hornblower's. The over-zealous friends of Watt, who, in a spirit so contrary to that of the great man himself, have sought to exalt his fame at the expense of another's, have charged Hornblower with pirating the principle, from surreptitious information of Watt's experiments; but no proof was ever given of the accusation. It is not only highly improbable in itself, but is altogether negatived by the fact that the originality of the invention on Hornblower's part was expressly admitted by Watt himself. It may therefore be concluded that the discovery of the expansive use of steam—one of the most important and valuable principles in the whole range of practical science—was original both with Watt and Hornblower; and, although Watt has established the priority of the idea, the first publication of it to the world was made by Hornblower in the double-cylinder engine.

After some years of delay, Hornblower proceeded to manufacture his engines in Cornwall; and the miners, perceiving that the double-cylinder engine acted tolerably well, took advantage of it somewhat largely; and in some cases endeavored to make it supersede Watt's single-cylinder expansive engine, which had also then been brought extensively into use. But as it was impossible to make Hornblower's engine work well, without using Watt's separate condenser, invented in 1769, the competition could not be kept up, and the double-cylinder engine consequently for a time fell into disuse.

Both Watt and Hornblower had failed to perceive that, to work the principle of expansion to its full advantage, it was necessary that the steam should be admitted to the cylinder, in the first instance, at considerable pressure. Down to the year 1814, the pressure of the steam in the Cornish engines never much exceeded that of the atmosphere: and so little economy resulted in practice from the application of expansion with this initial pressure, that it was found scarcely worth using at all; indeed, after Watt's immediate connexion with the district ceased, expansion was rapidly becoming disused and forgotten. The merit of rescuing it from this neglect belongs to two Cornish men—Richard Trevithick and Arthur Woolf—who, both about the same time, introduced into their native district the true means of advantageous expansion, namely, the use of high pressure steam. Trevithick applied this to Watt's single-cylinder engine; Woolf applied it to Hornblower's double-cylinder engine. The two forms of engines thus for the second time became rivals, and competed well with each other for many years; but it is only with Woolf's modification that the author is now concerned.

Woolf published his invention in 1804, while residing in London. It consisted simply in the application of high-pressure steam to Hornblower's double-cylinder engine, which he also made double-acting, to fit it for rotary motion, after the example set by Watt long before. It is obvious, therefore, that the name "Woolf's engine," by which it is so often designated, is quite erroneous. The engine is entirely and solely Hornblower's invention, and there is no more ground for calling it Woolf's than for calling the present Cornish engine Trevithick's: for Trevithick made the same change with this latter engine that Woolf did with the former, yet no one would, on that account, think of disconnecting Watt's name from his own engine; and on the same ground Hornblower ought not to be deprived of the credit which the association of his name with his own invention should secure to him. Woolf's ideas respecting the laws of the expansion of high-pressure steam were very crude, and it is difficult to conceive how a man of such excellent practical knowledge could have deluded himself into the belief of theories so palpably absurd as those he laid down, upon which he based his statements as to the proposed advantage to be derived from the use of high-pressure steam. But, although he was so essentially in error on points of theory, he was not wrong in foretelling that much advantage might ultimately be gained by the use he proposed to make of high-pressure steam; as was proved beyond a doubt when his engines came to be fairly tried. His strong point was, skill in mechanical detail; and his improvements of the engine in this respect were almost innumerable, for there was scarcely a single part which did not receive some beneficial alteration at his hands. Woolf's first engine was erected in 1806, at Meux's brewery, in London, to which establishment he was engineer, and subsequently others were fixed in various manufactories; but these did little more than serve him as experiments, until 1813, when he returned to reside in Cornwall. Here he found a wide field open for his improvements; he entered in earnest into the manufacture of the engines, and they were highly successful. The new doctrine of high-pressure steam produced quite a revolution in the consumption of fuel there; for he at once raised the duty from about 20 millions, at which Watt had left it (that is, 20 million ℔s. raised one foot high with the consumption of one bushel, or 94 ℔s. of coal), to between 50 and 60 millions, thereby saving two-thirds of the fuel employed.

But though Woolf was so successful, the Cornish engineers shortly began to see that Trevithick's plan of using high-pressure steam expansively in the single-cylinder engine promised equally good results with Woolf's, and at the same time got rid of the objectionable complexity of the double-cylinder arrangement. Trials on a large scale, in which even Woolf himself was persuaded to assist, soon demonstrated this to be true. The more expensive construction began to be abandoned in the mines, and the Cornish engine gradually settled down into its simplest form, namely, a single engine on Boulton & Watt's construction, but with Trevithick's high-pressure steam and high-pressure boiler; which form it has retained to the present day. Thus, although the double-cylinder engine was the first in which the principle of expansion was originally introduced to the world, and about thirty years afterwards was also the first in which this principle was made effective and advantageous, yet, in both cases, it was ultimately superseded by the more simple form of engine.

It remains now to give some account of the third era of prominence attained by the double-cylinder engine, in its revival at the present day. In this revival many modern engineers have aided; but the author considers it the safest course to confine himself to the statement of his own experience, leaving it to others to give an account of what they may have done.

In 1848 the Lambeth Waterworks Company, on the advice of their engineer, Mr. James Simpson, took the bold measure of proposing to remove their source of supply to the banks of the Thames at Long Ditton, above the tide way; and, as a part of this scheme, it became necessary to force the water, by steam-pumping power, along a cast iron main, 9 miles long and 30 inches diameter, from the source to the reservoirs at Brixton Hill. This problem was a difficult one, no experience on so great a length of large main having then been obtained. The great mass of water in motion along the main, combined with the fragile nature of the cast iron, rendered it essential that the motion should go on in the most equable manner, and that concussions or irregularities of pressure should be as much as possible avoided, otherwise frequent fracture of pipes, fraught with serious consequences to the district they passed through, might be looked upon as almost certain. At the same time, from the large steam power required, it became necessary that all possible improvements in regard to economy of fuel should be adopted. At that time the Cornish single-cylinder expansive engine, which had been introduced into London by Mr. Wicksteed, had been somewhat extensively tried for waterworks purposes, and had justified its well known Cornish reputation for economy; but as grave objections appeared to present themselves to its use in this case, on account of the irregularity of the single action, it was determined to ascertain whether the other form of expansive engine, the double-cylinder, would not prove more applicable; and since the importance of the case required the most careful consideration, the author was commissioned, in conjunction with Mr. David Thompson, to investigate the subject generally, with a view to the advantageous attainment of the desired economy.

In commencing this investigation it was found that the double-cylinder engine had already been to some extent revived, and that modern examples of it, some of considerable size, were working in various parts of the country. These were visited, and their action carefully examined; but it did not satisfactorily appear that any engines then met with were sufficiently favorable instances of the application of the expansive principle. The expansion had not been carried to a sufficient extent to produce great economy, nor arranged in the best manner to attain equality of motion; and the arrangement of the valves and passages were generally so defective as to cause great loss of power and waste of fuel. Notwithstanding these unfavorable results, however, an attentive study of the principles of the engine led to the conclusion that, with a well-considered design carefully carried out into practice, the double-cylinder arrangement promised not only to be eminently suited to the case in question, but also generally to offer a more beneficial application of the principle of expansion to engines of rotary motion than could be attained with a single-cylinder. In accordance with these views, when the Lambeth Waterworks Extension scheme was carried into effect, four large double-cylinder engines were designed of 600 total horse power, the working of which has fully justified the expectations entertained of their advantages; their use has been speedily and largely extended to other cases; and the soundness of the principle on which they were constructed may now be said to have been fully proved.

The general theory of the double-cylinder engine is so well known, that it is unnecessary to repeat it here: the author proposes, therefore, to confine his remarks to such points as are of interest and importance in elucidating the advantages of this form of engine.

In the first place, in comparing the double with the single-cylinder engine, it is a mistake to suppose that there is any theoretical advantage on either the one side or the other, in regard to the economical effect of the expansion. It was shown by Watt, in an ingenious way, at a very early period, and it is demonstrated in an appendix to this paper, that theoretically, if the steam be expanded to the same extent, the economical advantage to be derived from the expansion will be precisely the same, which ever form of engine be adopted for the application. And it further results from the principles of the engine, that, for a given initial pressure of steam and a given degree of expansion, the power of the engine, measured by the work it will do in each stroke, depends on the size of the large cylinder only, and is precisely the same as that produced in a single expansive cylinder of the same content. The small cylinder has no effect in adding power, but is merely an appendage, useful only for modifying the arrangement of the expansion and equalizing the steam's action during the stroke.

The important objection, however, against carrying expansion to any great extent in a single-cylinder for rotary motion, is the great irregularity of pressure at different parts of the stroke. For example, if the steam be expanded in a single-cylinder to six times its original volume, by cutting it off when one-sixth of the stroke has been passed over, the motive force acting on the piston will be six times as great at the commencement of the stroke as at the end.

It is evident that the effect of the great excess of pressure will be to give a heavy blow to the piston at the beginning of every stroke, which must produce violent concussions through the whole of the machinery; and tend to produce much mischief and inconvenience in the working. In proportion as a greater degree of expansion is used, the evil will be greater. For example, if ten times expansion be used, the force of the blow at the commencement will be 303, while the mean pressure in the cylinder is still only 100, as before. For this reason in single-cylinder engines it has been found difficult to carry the degree of expansion, and the consequent economy, to the same extent with the rotary as with the single acting pumping engines. In the latter, the piston and all its connexions are free to move under the action of the steam pressure; and therefore the excess of pressure at the commencement of the stroke is at once absorbed in giving velocity to the mass, and does no further harm. But in a rotary engine, the piston being controlled in its motion by the crank and fly-wheel, resist the violent impact, which occurs at the point when it is least able to give way; and the consequence must inevitably be a violent strain, repeated many times every minute, which must ultimately have a prejudicial effect upon the machinery. The advantage of the double-cylinder engine is, that it mitigates this evil; for when its principles are properly understood and applied, it enables the economical benefit of a high degree of expansion to be obtained with much less irregularity of pressure than in the single-cylinder.

In the original double-cylinder engines of Hornblower and Woolf, the steam was allowed to act first in the small cylinder, at full pressure, throughout the whole of its length, and then to expand into the larger one; the proportionate cubic content or the two cylinders thus defining the degree of expansion made use of: and, it is believed, that this method of working was that most commonly used down to the time of the investigations already referred to in 1848. But an inquiry into the principles of action of the engine, shows that it is most advisable not to allow the steam to enter the small cylinder during the whole stroke, but to cut it off after a certain portion of the stroke has been passed over, and to allow the expansion to commence at that point. And it is an important fact, which the author believes not to have been known until published by himself and Mr. Thompson, in 1851, that there is a certain point of the stroke, depending on the degree of expansion made use of, at which it is more advantageous to cut off the steam than at any other; for the reason that the irregularity in the motive power, which it is so desirable to mitigate, is then reduced to a minimum.

For example, if the mean motive power of the engine be represented by 100, and the extent of expansion adopted be six times, then, in a single-cylinder engine, the initial blow at the piston at the commencement of the stroke, as previously stated, will be represented by 215. In a double-cylinder engine, if the steam is allowed to enter during the whole of the stroke of the small cylinder, the initial blow will be the same in amount as in the single-cylinder engine, namely, 215; the duration of the blow, however, is only momentry, as compared with the first example, where it continues through one-sixth of the stroke. But if the steam is cut off in the small cylinder at 41 per cent. of the stroke, the initial blow is reduced to only 140: and this is the minimum blow that can be obtained with the expansion of six times; for on cutting off earlier, at 25 per cent. of the stroke, the initial blow is increased again to 161.

It thus appears that, as regards a degree of expansion of six times, if this expansion be effected in a single-cylinder, the machinery of the engine will have to bear a sudden blow at the commencement of the stroke, as much as 115 per cent. greater than the mean force due to the effective power of the engine. Next, if a double-cylinder engine be employed, and the steam be allowed to enter during the whole stroke of the small cylinder, but little improvement is effected: the blow at the commencement is as great as in the single-cylinder, only lasting a shorter time. But if the same engine be so arranged that the steam is cut off in the small cylinder at the proper point of the stroke, the initial blow may be reduced from 115 per cent. to only 40 per cent. in excess of the mean force; and thus a real and most beneficial improvement may be effected in the action of the engine. This most advantageous point of the stroke for cutting off, the author determined by calculation, in an appendex to his paper, which showed that it varies with the extent of expansion adopted in the engine.

The following Table shows the best point of cut off under various degrees of expansion, with corresponding results in the double-cylinder and single-cylinder engines. The first column gives the number of times the steam is to be expanded; the second shows the percentage of the stroke at which the steam will best be cut off in the small cylinder; the third, the corresponding proportionate area of the small cylinder in percentage of the large one, the length of stroke being the same in both; and the two last columns show the comparative advantage of the double over the single-cylinder engine, in respect to the excess of the initial blow over the mean motive force. The calculation of these results is explained in detail in the appendix.

Table, showing best point of cut off in double-cylinder engine with different degrees of expansion, and comparative initial blow in double and single cylinder engines.

Number of times steam is expanded.	Best Point of Cut off in small cylinder. Percentage of stroke.	Capacity of small cylinder in percentage of large cylinder.	Comparative Initial Blow; the mean motive force being 100	
			Double cylinder engine.	Single-cylinder engine.
	Per cent.	Per cent.		
4 times	50	50	126	168
6 times	41	41	140	215
8 times	35	35	151	260
10 times	32	32	161	303

A comparison of the two last columns shows that, for a high degree of expansion, such as eight or ten times, the excess of the initial blow over the mean force of 100 is less than one-third as great in the double-cylinder as in the single-cylinder engine. It is clear, that the amount of this initial blow is the maximum strain on the whole of the machinery, by which the steam power is transmitted from the piston to the fly-wheel, and it, consequently, determines the strength of the various parts necessary to resist this strain. Hence, in proportion as the initial blow can be reduced, all these moving parts are required to be less massive in construction; and all are subject to much less violent causes of fracture and derangement in their working.

Another point of improvement, to which the attention of the author and Mr. Thompson was prominently directed, was the arrangement of the valves and steam passages in the double-cylinder engine. In the engines they had the opportunity of examining, the system of valves commonly used was not only complicated, inconvenient, and expensive in construction, but also wasteful in action; the great size and disadvantageous arrangements of the passages caused considerable waste of steam, and consequent loss of power and fuel. There is a pecularity in the double-cylinder engine, in its requiring a pipe or passage of some kind, through which the steam must travel, from one end of the small cylinder to the opposite end of the large one; and this passage should evidently be as small in content as possible, consistently with allowing the free passage of the steam. When the communication is opened at the end of the stroke, the steam, passing from the small cylinder, has to expand and fill this passage before it enters the large cylinder; and if the passage be large, the steam must necessarily suffer a reduction of pressure in so doing, which must seriously diminish its effective action on the large piston during the future stroke, and so cause much loss. In some engines, the author found this loss so great as to waste nearly half the power of the engine; and even in the best that were examined, such a considerable percentage of loss occurred, as almost to neutralize the benefit of expansion altogether.

Much attention was therefore given to this point in designing the new engines; and it was found essential to the success of the engines that some arrangement of valves should be adopted which should satisfy the following conditions: first, that they should be of the simplest possible character, and free from liability to derangement; second, that they should admit of the steam being cut off from the small cylinder at such a point as might be necessary to secure the required regularity in the motive power of the engine; and, third, that they should give the clearance spaces the smallest content possible, and, in particular, should allow the passage between the two cylinders to be direct and unimpeded, and of no larger capacity than was absolutely necessary for the passage of the steam. It was further found conducive to economy that the passage between the two cylinders should, if possible, never be opened either to the high-pressure steam or to the condenser, and should, moreover, be carefully protected from cooling.

A construction of valve was accordingly introduced which combined all these conditions, and was found in its practical working to be very satisfactory.

Proc. Inst. Mech. Engineers

Gun Cotton—Important Application.

From the London Mining Journal, No. 1500.

At the Royal Institution, on Friday evening, the 13th inst., the recent improvements in Gun Cotton again formed the subject brought to notice, the lecturer on the occasion being Mr. Scott Russell. In the previous lecture on the subject, by Mr. Abel, the chemical history of gun cotton, and the nature of the improvements in its manufacture recently made in Austria, were more especially dwelt on; while Mr. Russell principally directed attention to its use. He stated that at a late meeting of the British Association a committee was appointed, of which he was a member, to investigate the nature of the great improvements which it was known had been made in Austria on gun cotton by General Lenz, but which were there kept a profound secret by the Austrian Government. One of the first things the committee did was to write to General Lenz, to request him to come over and acquaint the committee in what his improvements consisted. This impudent request—as Mr. Russell admitted it to have been—excited a commotion in Vienna, but the young Emperor, against the advice of his counsellors, consented to allow the secret to be known, and Gen. Lenz came to this country, and informed the committee what it was. The object to be accomplished in the manufacture of gun cotton, to render it applicable, was to diminish the rapidity of its combustion, and its liability to accidental explosion. These objects have been fully accomplished, as was previously stated by Mr. Abel, principally by spinning the cotton into yarn before it undergoes the process of conversion into gun cotton; and it was shown by that gentleman that by mechanical alteration of the structure of the cotton fibre alone the explosive material may be placed under complete control, and that, as he said, you may do what you like with it. What can be done with gun cotton, and what cannot be done with it, and its merits, compared with gunpowder, as an explosive agent, were explained and illustrated by Mr. Scott Russell, who, though he commenced by mentioning some points of objection to gun cotton, finally removed them, and represented it to be *in every respect superior to gunpowder*, and to be capable of producing far more destructive effects.

As an illustration of the different degrees of rapidity of combustion attainable with gun cotton, he first ignited a thick yarn of the substance that was suspended round the gallery of the lecture-room, which burned along at the rate of about one foot in a second. He next took a pipe about a yard long, which flashed off in half a second; and another of similar length, of the most explosive kind, exploded instantaneously. A remarkable property of gun cotton, which constitutes an immense advantage in its use compared with gunpowder, is that it leaves no products of combustion behind and produces very little smoke. Another important advantage is, that it is not injured by moisture, and it may be wetted and dried again without impairing its power. This was shown by immersing a piece of gun cotton yarn in water, which was then squeezed out, and, though still damp, the outer surface rapidly ignited, and when the remainder was squeezed the cotton again ignited. This property is a great safeguard in the manufacture of gun cotton, for all the processes may be conducted under water, and it is afterwards dried. For ordinary practical purposes the rapidity of its explosion is reduced to an equality with that of gunpowder, in which state 1 ℔. propels a projectile from a cannon with the same velocity as 3 ℔s. of gunpowder. The recoil of the cannon when gun cotton is used is only two-thirds the amount of recoil with gunpowder; there is no fouling of the gun, which remains also comparatively cool; and the absence of smoke enables the gunners to continue firing in casemated batteries and between the decks of ships without annoyance. As an explosive agent, gun cotton possesses some remarkable pecularities, the causes of which have not yet been ascertained. A piece of loose gun cotton fired on a balanced scale produces depression, the whole force going off into the atmosphere without any recoil. But if the gun cotton be enclosed in a box or barrel, its explosive force in all directions is much greater than that of gunpowder. Some important experiments on a large scale have been tried with gun cotton made by a manufacturer of the substance in Suffolk. A pallisade was formed with the trunks of a number of large trees fixed in the ground close together, and a small box filled with gun cotton was exploded near them. A large gap was instantly made, the trees having been cut asunder quite straight, though at the top of the fractures the wood was splintered into matches. Its destructive effect was tried on two wooden bridges, which were completely splintered; but the most important experiment was the blowing up of a ship of 400 tons burthen by means of 40 ℔s. of gun cotton enclosed in a barrel and placed under water at a distance of 20 ft. The ship, it was stated, was completely blown to pieces.

The explosive force of gun cotton when applied to mining operations is equally effective. At the Clay Cross Collieries it has been found that a blast with 1 oz. of gun cotton will remove from 60 to 80 cubic feet of coal, and it has been applied at the slate quarries in North Wales with equal success. It is a great advantage in using gun cotton in mining or tunneling that the work is not impeded by smoke, which is a serious inconvenience when blasting with gunpowder.

Mr. Scott Russell attributed the remarkable diminution of recoil in explosions of gun cotton to the absence of solid matter in the products of combustion. In gunpowder the solid refuse amounts to about 60 per cent. of the weight of the powder, and the effort required to blow away this "rubbish," he considers, produces the additional amount of recoil. In gun cotton, on the contrary, there is scarcely any solid refuse, 25 per cent. of the products of combustion consisting of water; and he conceives that the expansion of this water into red-hot steam by the ignited gases during their explosion is a main cause of the great explosive force of gun cotton. Mr. Russell ventured also to speculate that the lower degree of heat caused by the explosion of gun cotton may be owing to the subsequent condensation of the steam into water. He admitted, however, that the causes of these and of other peculiar properties of gun cotten are involved in doubt, which he hoped the further investigations of the committee of the British Association would remove. He also mentioned that a commission had been appointed by the Government to consider the application of gun cotton to artillery.

Conversion of Salt Meat into Fresh: a further application of Dialysis,

By A. A. Whitelaw.

From the London Chemical News, No. [illegible]

As an appendix to the notice of my process for the utilization of brine (published in the *Chemical News* for March 26), I now beg to direct attention to a modification of that process, applicable to ships at sea, by which the quality of the meat supplied to the men may be much improved, and their food varied.

The salt meat is placed in a dialytic bag made of untanned skin, or other suitable material, and the bag filled nearly, but not quite, full of brine from the beef barrel. The dialyser is then placed in sea water, and the process allowed to go on for several days, till the meat and brine are sufficiently fresh for use, or till the brine in the dialytic bag is within 1° or 2° of Twaddle's hydrometer of the same strength of sea water. In this way, as the brine becomes freed from salt, the beef, which, by the action of salt, has been contracted, gives its salt to the brine in the bag, and so the process goes on, the beef expanding like a sponge, and gradually taking up a great part of the natural juice that it had previously lost in the salting process. In this way no loss of juice is sustained by steeping, and the brine left in the bags, after a nightly dialysis in fresh water, can be used for soup.

Thoroughly salted beef, without bone, takes up nearly one-third its weight in juice, and this absorption takes place gradually as the strength of the brine in the dialyser becomes reduced.

A Comparison of some of the Meteorological Phenomena of JULY, 1864, *with those of* JULY, 1863, *and of the same month for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	July, 1864.	July, 1863.	July, for 14 years.
Thermometer—Highest—degree, .	94·00°	88·00°	100·50°
" " date, .	31st.	15th.	21st, 1854.
" Warmest day—Mean, .	86·17	81·67	91·30
" " " date,	31st.	26th.	21st, 1854.
" Lowest—degree, .	60·00	65·00	53·00
" " date, .	22d.	17th.	2,3, '62; 3' 57.
" Coldest day—Mean,	66·33	70·50	59·7
" " " date, .	25th.	17th.	3d, 1857.
" Mean daily oscillation,	15·73	10·81	15·87
" " " range, .	3·81	3.22	3·77
" Means at 7 A.M., .	72·39	74·40	73·69
" " 2 P.M., .	83·48	80·24	83·43
" " 9 P.M., .	76·97	76·32	76·24
" " for the month,	77·61	76·99	77·79
Barometer—Highest—Inches, .	30·066 in.	29·988 in.	30·212 in.
" " date, . .	18th.	18th.	5th, 1859.
" Greatest mean daily press.	30·038	29·982	30·197
" " date, . .	18th.	18th.	5th, 1859.
" Lowest—Inches, .	29·518	29·524	29·443
" " date, . .	2d.	9th.	19th, 1851.
" Least mean daily press.,	29·585	29·544	29·462
" " date, . .	2d.	9th.	30th, 1856.
" Mean daily range, .	0·114	0·079	0·094
" Means at 7 A.M., .	29·784	29·800	29·838
" " 2 P.M., .	29·747	29·780	29·809
" " 9 P.M., .	29·783	29·807	29·826
" " for the month, .	29·771	29·796	29·824
Force of Vapor—Greatest—Inches,	0·860 in.	0·812 in.	0·983 in.
" " " date, .	2d.	14th.	26th, 1854.
" " Least—Inches, .	·255	·399	·255
" " " date, .	22d.	22d.	22d, 1864.
" " Means at 7 A.M., .	·540	·663	·608
" " " 2 P.M., .	·537	·660	·604
" " " 9 P.M., .	·592	·684	·636
" " " for the month,	·556	·669	·616
Relative Humidity—Greatest—per ct.,	97·0 per c.	92·0 per c.	97·0 per ct.
" " " date,	25th.	13th.	often.
" " Least—per ct.,	27·0	43·0	26·0
" " " date, .	23d.	23d.	23d, 1856.
" " Means at 7 A.M.,	67·2	77·7	72·4
" " " 2 P.M.,	46·6	64·6	52·8
" " " 9 P.M.,	63·2	75·2	70·0
" " "for the month	59·0	72·5	65·1
Clouds—Number of clear days,* .	8·	1·	6·8
" " cloudy days, .	23·	30·	24·2
" Means of sky cov'd at 7 A.M.,	62·9 per ct.	90·0 per ct.	59·5 per ct.
" " " " 2 P.M.,	58·7	80·3	59·1
" " " " 9 P.M.,	35·8	50·6	41·6
" " " for the month,	52·4	73·7	53·4
Rain.—Amount, . .	3·742 in.	5·690 in.	3·807 in.
No. of days on which Rain fell,	8·	19·	11·1
Prevailing Winds—Times in 1000	s 75° 5′ w·263	s10°53′w·157	s60°36′w·155

* Sky one-third or less covered at the hours of observation.

JOURNAL

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FOR THE

PROMOTION OF THE MECHANIC ARTS.

SEPTEMBER, 1864.

CIVIL ENGINEERING.

On the Testing of Chain Cables. By FREDERICK ARTHUR PAGET, Esq., C.E.

From the Journal of the Society of Arts, No. 596.

(Continued from page 95.)

THE apparent increase of ultimate strength through successive breakages, thermo-tension, and much of the high static breaking strength acquired through cold rolling, and cold hammering, even through wire-drawing previous to annealing, are referable to an increase of hardness, to an increase of the difficulty of the gliding to and fro, to a resistance to the inter-mobility of the particles, to, in one word, a diminution of elasticity. If the numerous experiments that have now been made on iron do prove any thing, it is that the breaking strength does not indicate the quality—the breaking strength must be taken conjointly with the elongation. The true measure of the mechanical value of wrought iron is simply the sum of the products of the successive loads and the increments of elongation—in other words, the resilience of the bar or the deflection of the beam, or the work performed in producing the stretch or deflection. We thus see the value of Poncelet's symbols T_0 and T advocated with such ability in England by Mr. Mallet. Upon the just balance of strength of fibre, or high breaking strength, and extensibility or ductility, depends the mechanical or structural value of iron.

The Navy test for chain cables is stated to be the result of a number of careful experiments by the late Sir Samuel Brown, and it was adopted by the Admiralty in 1831, when chain cables were fairly established in the royal service. The test adopted by the Fench Navy is almost exactly the same, and in Russia and the States it is exactly the same, as both those countries use our own measures and weights. Every chain cable is proved by a gradually applied stress of 630 lbs. for each circular one-eighth of an inch of the area of the bolt of which the cable is made or 11·46 tons to the square inch on each side of the link.

Assuming that a link is subjected in practice to a tensile stress, and as the proof strength is generally fixed at double the working stress, this would correspond to nearly $5\frac{3}{4}$ tons on the square inch. There is thus a very close correspondence between the working stress assumed for chain cables and the Board of Trade limit of 5 tons to the square inch, imposed about 16 years ago, for both the tension and compression of the wrought iron of railway structures. The chain cable of a ship is also evidently subjected to impulsive forces. It is true that a ship, when struck by a sea, in most cases nearly lifts the weight of her chain, the catenary curve of which thus acts as a kind of water-brake; but a very heavy sea must occasionally bring a sudden pull on the cable, and in shoal water the sudden strain must be almost solely taken up by the resilience of the cable, or rather by the deflection of the series of beams composing the cables. Much security is, however, afforded by the fact that a cable is generally only strained during a brief interval of time. But few cables can stand a sudden nip at the hawse-pipe; and we thus see that lateral as well as longitudinal strength is occasionally required in a cable.

If two one-inch diameter cable bars of average quality, and say, each ten feet long, be put into the hydraulic press generally used for testing cables, the following appearances will probably be observed:—If new, they will take a very slight set under a stress of about $1\frac{1}{4}$ ton to the square inch, but if this stress be gradually increased, and alternately eased off and put on several times, the set will not increase until the true elastic limit or proof strength of the material be exceeded. In our case this limit will probably be 12 tons to the square inch, which is thus higher by a little more than half-a-ton than the 11·46 tons navy test. At the Admiralty proof stress, each of the bars will have a probably total elongation of more than one-twentieth of an inch, and a permanent set of six thousandths. Beyond this strain the set will very rapidly increase up to, perhaps, two inches, when the bars will break under a load of 24 tons to the square inch. But the phenomenon the most important in its consequences, consists in the contraction of cross sectional area undergone by the bar through the stretch. According to a theoretical investigation by Poisson, the relation of the contractions to the longitudinal elongation should be $\frac{1}{4}$; and Wertheim's experiments led him to believe that this relation should be $\frac{1}{3}$. Cauchy, Stokes, Maxwell, Rankine, and Lamé, have also mathematically investigated this question, and have arrived at results differing from those of Poisson, which were founded on a special atmoic hypothesis. But the permanent sets that show themselves in ductile bodies, like annealed iron, under very slight loads, and the so-called frictions observed by Dr. William Thomson in metals under tension, would cause this relation of contraction to elongation to differ for every different state of a metal. To Kirchhoff is due a remarkably important investigation carried out in 1859, into the relation of the contraction to elongation under tension of hard steel wires—which may be said to approach the nearest to the ideal of a body possessing equal elasticity in different directions. His experiments, conducted with great delicacy, gave a relation of cross sectional contraction to elongation of 0·294. As we have seen, according to the Admiralty tables, a one-inch cable bolt ought to have an ultimate breaking strength of not less than 21 tons 8 cwts. to the circular inch, or more than 27 tons to the square inch, and the link ought only to break at 34 tons. It is, however, very seldom that these strengths are obtained in practice. The ultimate elongations of the bars or the cables are not stated in the Admiralty tables. General Morin relates that the fine charcoal iron, made at Guerigny by the French government, expressly for chain cables, sometimes elongates even more than one-fifth of its original length before breaking, and this amount is probably the utmost that it is possible to give to wrought iron bars.

When the cable itself is placed under the dead pull of the press, it is tested in three different ways. It is first strained up to 11·46 tons in the square inch sectional area across the double section of the link. While for about three or four minutes under this stress, the cable is subjected at different parts of its length to blows from a round-faced hammer. Different sized hammers are adopted in proportion to the size of the chain, and each fathom generally receives one blow. Each link is then carefully examined. Two or three links are broken up to detect, by its bluish tinge, if the iron has been at all burnt in the working, and also to make some estimate of the quality of the iron from the surface of the fracture, and the other appearances known to engineers. Some differences of opinion also exists, both in France and in England, as to the amount of security afforded by these tests, and whether the test of 11·46 tons on the square inch, and more especially the blows of the sledge, do or do not injure the cable. In 1855 it was attempted to introduce a compulsory government test in France for the chain cables of merchant vessels. A letter was addressed by M. David, an influential chain cable manufacturer at Havre, to the then imperial minister of public works, advocating a compulsory test, from motives of humanity to the ships crews, and of public economy. A system of periodical re-testing, for every ten or twelve years that the cable had been at work, was also proposed. The attention of the then minister of the French marine was directed to the statements put forth, and Admiral Hamelin ordered an official investigation of the question. The results shown forth in the report would appear to have proved—at any rate to the satisfaction of the Imperial administration, that—1st. "The proof test of 17 kilogrammes or even of 20 to 21 kilogrammes per square millimetre of section of the link, is not enough either to prove the good workmanship of the cables or the quality of the iron employed; 2d. That a higher proof than 20 to 21 kilogrammes cannot be applied several times to cables without affecting their quality; 3d. That the differences of useful effect between different presses often lead to error with respect to the absolute value of the tension employed. . . . . The sum total of these results therefore shows, continues the minister, that, on the one hand, an increase of the proof test would not be of much effect in detecting bad materials and workmanship, and on the other that it would be dangerous to increase the test. The required security can only be obtained in a well understood system of manufacture; and, therefore, besides the test in the press, it is necessary to scrupulously choose the special quality of iron required; to accurately examine each individual link after the testing; to break up any questionable link; and to choose the most skilful and trustworthy operatives." In one word, the minister of the marine did not consider a government inspection of chain cables intended for the French merchant service as a practicable thing. It is to be remembered that all the chain cables for the Imperial navy are manufactured by the government.

Now there can be no doubt that the proof of 11·46 tons to the square inch is not enough of itself to test the quality of the workmanship, or, more definitely, the perfection of the welds. For this reason Mr. R. Bowman advocated before the 1860 Committee an increase of the test. It is clear that, as the sides are only tested up to little more than 11·46 tons, and as they would break at only, say, 24 tons to the square inch, less than one-half the sectional area of the iron would stand the test if applied only tensionally. As, however, through the cross-bending strain at the two ends, the link slightly tends to assume the shape of a lozenge, the weld is more severely tested than would at first appear. There is a certain difficulty in detecting a bad weld, upon the nature of which some practical light has been thrown by some experiments of Mr. Kirkaldy's on bars grooved round their circumferences. The matter had been previously investigated by the writers on elasticity, but Mr. Kirkaldy was the first to practically test the question. Bars grooved at any particular part down to a given diameter, gave a much higher ultimate breaking strength than bars of a diameter all through equal to that at the reduced part of the gooved bar. The wider parts on each side resisted the tendency to draw out, and a great apparent strength was thus obtained. The extent of this apparent gain was as much as 37½ per cent. in some of the pieces, while the average gave 18·63 per cent. in favor of the grooved specimen. Here again we see the falsity of taking merely the breaking strength into account, for although the breaking strengths were thus increased, the elongation, and the contraction of area attendant on elongation, were proportionately less. It will thus be seen that a bad weld may be impaired by a strain in excess of the elastic limit due to the quality of the iron and the cross sectional area of the solid metal, and that, although it is thus injured, it may not show signs of the injury. On the other hand, some security is given that a bad weld may be detected, from the fact that rolled iron is well known to be somewhat hardened by being hammered, and the welded-up side of the link would thus be less extensible than the opposite parallel side, and would thereby be rather more strained. It is evident, however, that though the test can scarcely be too high for the welds alone, the proof of more than fourteen tons to the square inch, proposed by M. David, would clearly be too high for the cable. M. David, indeed, stated that he tested his cable up to this amount, but it appears that the pressure he used was not accurately measured. Indeed, there is no doubt that very few cables would stand the ordinary proof if repeated sufficiently often, or if it were put on and eased off a succession of times, upon the plan shown by Dr. Rankine. As it is, the permanent set taken by cables is, on an average, from 4 to 6 feet in 90. But the best proof that this single application of the test for a short time does not injure good chain cables, is seen in the fact that it has been adopted all over the world for more than thirty years. We are, however in a dilemma. To increase the proof would evidently be to injure the link, while the detection of a bad weld has, in any case, to encounter the difficulties just mentioned. These questions can only be met by a most careful inspection of each individual link. The quality of the iron can also be very closely tested by breaking up two or three links. The most searching tests, however, are the hammer blows given while the chain is under tension. Adapting a well-known and excellent illustration, this will be at once evident when we remember that a 1¾-inch chain cable, made of glass, would give the same ultimate gradually-applied breaking strength as a one-inch iron cable—but it would not be likely to stand the hammer test. On the other hand, a cable of india-rubber, although not to be broken by the hammer, would at last be torn in two by the press. In fact, the hammer test approaches nearer than any other to the kind of work that will have to be done by the cable when at sea. Besides, the mere form of a chain renders it, *per se*, liable to continual shocks and jerks, and this must be encountered by a special quality of material, and that this material has really been used must be shown in the proving house.

Mr. Pope, the surveyor for Lloyd's at Liverpool, gave it as his opinion, before the Committee of 1860, that the navy test was too high, and had a tendency to injure the chain. This might be true for a chain of a bad material, but not for a chain made of iron with the high elastic limit that should alone be used for chain cables. He proposed to test a short piece to destruction, and then to test the entire chain up to half the Admiralty proof. Apart from the expense and destruction of material by this proceeding, there can be little doubt that half the usual test would not detect all the bad welds, and the distinctive peculiarity of a cable consists in the fact that a single bad weld is sufficient to cause the entire loss of the chain.

As we have seen that a cable consists substantially of a series of small curved beams, it would be only a natural inquiry to ask why the sum total of their deflections, represented by the temporary elongation of the cable, and why the total permanent set should not be both registered, and be both taken into account when estimating the quality of the cable. There are, however, several influences that would greatly disturb an accurate deduction. It might, at first sight, be supposed that the defective welds would elongate in the inverse ratio of their areas of solid metal to that of the links. This, as we have seen, is not the case, and even if it were the case, the action would affect the deductions therefrom by variable and uncertain quantities. The links will also bed against each other to an amount given by the hardness of the iron. There can be no doubt that the extension must be taken into account with the breaking weight, when the quality of a bar has to be estimated. But even with bars, this varies considerably, not merely in different qualities, but also, as was shown by Mr. Kirkaldy, in specimens of the very same brand. These results were also obtained under tensional stresses alone, and when we come to the combination of transverse, tensile and directly compressive stresses to be found acting on a link, the varied ways in which these stresses act on varying qualities of iron would scarcely render the deductions from the elongations and set sufficiently trustworthy. Again, to take an extreme case, if one half of, say, 50 fathoms of cable were made of a very bad kind of iron, and the other half of a very good quality, it would be difficult to draw any right deduction from these appearances. As it is, however, the permanent set is generally registered.

There is probably no metal the strength of which is influenced in such a remarkable way by temperature as iron. As M. Baudrimont showed in 1850, the tenacity of iron is less at 100° C. than at 0° C., but at 200° it is greater than at 0°, and these results have been exactly confirmed by Dr. Fairbairn in some experiments on boiler plates, communicated in a paper to the British Association. At yet higher temperatures this tenacity is of course diminished; and Seguin has shown that iron, the tensile strength of which could be represented by 100 at 10° C., had this tenacity lowered to 90·5 at 370° C., and to 58·7 at 500° C. In the royal dockyards of Woolwich and Portsmouth, the atmospheric temperature during the testing of each anchor or chain is carefully noted, although the proving houses themselves are kept at a mean temperature of 56° Fahr. by means of stoves, which also thus save the water pipes from freezing. This temperature of course falls a little during the winter and rises in summer, as the heat in the shade generally varies in England from about 76° to perhaps 34° Fahr. The action of frost on iron has not been completely investigated; and Dr. Percy recommends that some acurate experiments on the question be undertaken by the Institution of Civil Engineers. The daily observation of practical men has, however, as in so many other cases, preceded the deeper investigations of science. All workmen know that their tools, such as picks and chipping hammers, which have to undergo percussion in frosty weather, are then more liable to get broken. All chains are well known to be more subject to snap under the same circumstances. There is always a notorious increase of accidents through breakages, both in the permanent way and rolling stock, of railways during frosty weather. It is stated that during the severe winter of 1860–61, 498 rails were broken on the Chemin de Fer de l'Est, from the 11th December to the 31st January inclusive. No less than 258 were broken from the 21st to the 25th of January, during which period the thermometer descended to —7·8° and even to —16° centigrade. General Morin relates that during the northern campaigns of the first empire the artillery veterans used to believe that wrought iron was subject to freezing, and after the long winter bivouacs they never began their day's march without striking the gun-carriage axles in the direction of their length, and the vibration thus produced was said to "thaw" the iron. An intense cold is also said to have enabled the French garrison of Hamburg to disable the cast iron siege guns, by knocking off the trunnions before evacuating the place. Mr. Lenox stated, in evidence before the 1860 Committee, his belief that a cable would stand a test in warm weather that it might not in cold. The crews of the fishing vessels on the coast of Nova Scotia find that the cold renders their cables so brittle that a length of hempen cable is used for the portion out of the water, while the anchor end is kept from the vicissitudes of the atmosphere by the usual average temperature of the sea. A few experiments made by Mr. Kirkaldy showed that the breaking strength of a bar is slightly reduced by freezing when a gradual breaking load was applied, but that this difference between a frozen and an unfrozen bolt is much more increased by a suddenly applied load, being 3 per cent. less when frozen. The usual way adopted by French engineers to test rails is, as we have seen, to prove a percentage of the lot by means of a falling weight. Some tests were carried out a few years ago by M. Couche, on a number of rails, of very good quality, from the Anzin works. The monkey weighed 300 kilogrammes, and the distance between the supports was 1m. 10. When the thermometer varied from —4° C. down to —6° C. the weight had only to be raised, in an average of twelve experiments, to a height of 5 ft. 6 ins. in order to break the rail; but when the thermometer rose from + 3° to + 8° C., then the weight had to be lifted for a fall of 7 feet 9 inches. Similar experiments conducted in 1860, showed that a difference of temperature from — 4° to + 5° centigrade was sufficient to greatly influence the height of fall necessary to break the rail. It is not unnatural to suppose that the particles of iron, after being worked at a heat and allowed to cool and set at a medium temperature, should, when that temperature is lowered, get into a state of mutual strain; or that any initial mutual strain should be thus intensified. The toys made of suddenly cooled glass, known as Prince Rupert's drops, are exaggerated instances of a similar action. The outside portions of a bar of whatever size, would evidently cool and consequently contract first of all. The inside portions would also at last cool, but, having kept the outside portions distended, when the inside does cool, it then becomes a question, to be determined by various circumstances, whether it would pull the outside shell into a state of compression, or whether the outside shell would draw the inside into fissures by tension. A somewhat similar explanation is given by Mr. Mallet of the rents caused in the interior of very massive forgings, and this state is probably always induced by the conditions of cooling in a small bar, but with, of course, a smaller range both as to size and temperature. In any case, it is apparent that a ductile, elastic material ought to be less affected by these doubtlessly complicated conditions of tensile and compressive strains. It is, therefore, probable that a hard, harsh, iron would be more affected by frost than a soft ductile iron, and also that the breaking strength of both qualities would be less affected by cold than their extensibility. It is even by no means improbable, though the fact would be difficult, or at any rate very expensive, to prove, that the breaking weight, or the elastic limit, or both, of iron, is or are different for every degree of heat. A bar is perhaps cooled down in the rolling shed to the medium atmospheric temperature of, say, 52° Fahr. At a lower temperature, at a temperature for instance, of 32° Fahr., its static breaking weight is increased, but its power of elongation under stress is probably diminished. At, say, boiling point, its breaking strength is diminshed, but its power of elongation is increased. These remarks to some extent meet the results of Baudriment and Fairbairn. Unfortunately, Baudriment has not recorded the elongations, and his experiments were made on wires only one millimetre in diameter when at a temperature of 16° C. Dr. Fairbairn did find that the elongations of plates increased very closely with the temperature, but his experiments are not sufficient in number to be taken as conclusive; and, as Dr. Percy remarks, many more experiments are required on the action of frost on iron. If it could be shown, for instance, that the crystals of iron expand to different degrees in their different axes, this would probably, *per se*, meet the scarcely-to-be-doubted fact that iron is rendered brittle by frost. As the chain cables of a ship are alternately exposed to the utmost extremes of atmospheric temperature, this question is here of peculiar importance.

(To be Continued.)

Spoked and Disk Wheels.

From the Practical Mechanic's Journal, June, and July, 1864.

(Continued from page 86.)

M. Martin having been engaged to assist in conducting the experiments made under the direction of M. H. Bochet, mining engineer, the first deductions from which are inserted in the second part of the *Annales des Mines* for the year 1858, the whole of which treat of the friction of rolling; but the second part, which is not yet concluded, will treat of the variations of those resistances which the action of the air produces; and lastly, every thing which concerns the questions of velocity, specific pressure, of lubrication, &c., &c.

M. Bochet having been requested to give information on this interesting and no less important subject, prior to the entire publication of his extensive researches on those points which relate to the action of air upon revolving wheels, has addressed M. Martin in the following terms:—

"It appears from the numerous and exact experiments which I have made with a dynamometer, to determine the resistance which is offered to the rotation of the wheels, that this resistance is made up of three different forms, as follows:

"First. The resistance due to the friction of the journals of the axle in their bearings; secondly, The friction resulting from the rolling of the wheel on the rail; thirdly, The resistance of the atmosphere upon all the surfaces exposed during rotation.

"If we compare one with the other, the total resistance opposed to spoked wheels on the one hand, and on the other that opposed to disk wheels of the same diameter, breadth of tyre, and weight, all other things being the same, as well as the velocity of rotation, the total resistance opposed to disk wheels is always much less than that offered to spoked wheels.

"This diminished resistance offered to disk wheels, comparatively with that to which spoked wheels are subjected (the two first elements of resistance above mentioned being identical in each case), although of immense importance at a slow rate of revolution, becomes of still greater importance as the velocity of rotation is increased.

"At a speed 400 turns per minute, the resistance of the atmosphere upon disk wheels is not more than half of that offered to those with spokes (this fact was obtained from observations made upon the wheels of a truck, whose diameter was 1 metre). This difference of 1 to 2 is not a variation which is produced from the sum of the resistances encountered, but that of the atmospheric resistance only; for in both instances the resistance of the journals in their bearings and the rolling friction of the wheels upon the rails remain constant; but when the wheels have only a light weight to carry, and especially if the journals are well lubricated, the two last mentioned resistances are considerably lessened; and above all, at a high velocity, they are much less than generally supposed; so that at a high velocity of rotation, the entire resistance offered to the revolution of a disk wheel is rarely if ever increased beyond the half of that offered to a spoked wheel of similar dimensions. It is also well to add, that the small disturbance of the atmosphere by disk wheels when traveling at a high velocity, is a special advantage, independently of the diminished resistance to traction which results.

"It may also be stated, that as the load becomes greater, and the lubrication of the journals becomes inferior, the rate of revolution diminishes in an exactly corresponding proportion, the total resistance encountered by the wheels always reverting towards an equilibrium; that is to say, as the load and friction increase, the velocity of rotation decreases; and as the load and friction decrease, the velocity of rotation increases.

"H. BOCHET"

These experiments were made with the special view to determine the results contained in the above letter. The same wheels were easily made either to present a disk or spokes to the resisting action of the atmosphere; for thin iron disks were prepared of a diameter equivalent to that of the spoked portion of the wheel; these were easily fixed on either side of the wheel, so as to make a plain disk surface; or when taken off, the spokes were left exposed as usual.

We think it well, in order to complete this interesting subject, to add some extracts from a paper by M. J. Poirée, Ingenieur des ponts et chaussés, published in the Transactions *de la Societe Ingenieurs Civile.* The experiments referred to as having been conducted by M. J.

Poirée, were carried out for the purpose of obtaining complete information upon every point which influences the traction of railway trains, such as the work in ascending inclines, as well as of passing over curves, the resistance to be overcome in the engine itself, its actual power, its consumption, the atmospheric resistance opposed to trains, &c., but strange to say, that the obstruction offered by this element to the rotation of the wheels, although it was inquired into, was not carried out so as to obtain any satisfactory results.

The atmospheric resistance opposed to various classes of trains may be stated approximately in the following order:—

To the traction of	ballast trains,	at 3k.2·10	per tonne.
“ “	goods trains,	at 4 1·2	“
“ “	passenger trains,	at 7 7·10	“
“ “	express trains,	at 10	“

These experiments were made with a Morin dynamometer, placed in the back part of the tender of the engine which drew the experimental train. The instrument was capable of measuring the opposing forces as high as 2300 kils.

An anemometer, destined to give the intensity and direction of the currents which acted upon the train, was placed above the tender. Lastly, watchmen placed on the line noted the natural and chief direction of the wind both before and after the train had passed.

In the whole of these experiments, the traction per tonne on the level varied from 3 kils. to 14 kils. The resistance of the atmosphere per square metre, as shown by the anemometer, varied from 0 to 57 kils. per square metre. In the last case, the train's lineal velocity was 20 metres per second, with a contrary wind. The influence of a side wind is very important. With a nearly constant velocity of travel 56 kilometres per hour, the load of 117 tonnes also remaining constant, the tractive force on a level was found on two different journeys to vary from 6k. 5 to 10k 5 per tonne.

In the first case, the wind acted upon the train precisely at an angle of 10° to its direction of travel; but in the second, it acted in opposition to it at 28°.

If the train is supposed to consist of six carriages, making a total weight of 80 tonnes, running at a speed of 12 metres per second, and acted upon contrary to its travel by the wind striking it at an angle of 45° with a velocity of 6 metres per second, it is found that the resistance due to the impact of the atmospheric currents upon the anterior portion of all the vehicles composing the train amounts to 3 kils. If the same wind, however, struck it in the direction of the trains motion, the resistance per ton due to the action of the atmospheric currents would only be 0k. 22. Upon a calm day, the resistance would only be 0k. 50.

These experiments have shown the wide differences which actually exist between the above numbers; but in this calculation neither the resistance that the atmosphere exercises upon the wheels, nor the friction of the wheels upon the rails, have been taken into account.

The Traction of Passenger Trains.—English engineers have adopted, and the authors of the "*Guide des Mecaniciens*" have recommended the following formula for this calculation:

$$R = 2k.72 + 0{,}094\,V + 0{,}00484\frac{NV^2}{T} \quad . \quad . \qquad (11)$$

R being the resistance per train tonne.
V the velocity per hour, in kilometres.
T the total weight.
N the total train surface exposed to the direct resistance of air.
2k.72 represents the co-efficient of friction.
0094 proportional to the velocity, expresses the resistance due to concussions, undulations, and vibrations of the permanent way.
000484 proportional to the square of the velocity is the resistance of the wind.

This formula re-produces the numbers found in M. J. Poirée's experiments, making

$N = 14$ metres for train of carriages;
$V = 7$ metres for express trains.

M. Martin had recourse only to the preceding when calculating according to the indications given by the anemometer, the resistance per tonne drawn, due to the impact of the air upon the anterior portions of all the vehicles composing the train which was found to vary from 3k.00 to 0k.5, has proved that the total resistances are very much greater, but that he had not taken into account the resistance of the atmosphere upon the wheel, or the friction on the rails.

Comparative weight of Spoked and Disk Wheels.—If we compare one with the other wheels of 0m 92 in diameter, they will be found to have the following weights:

Wheels	with single spokes,	133 kils.
"	with double spokes,	181 "
"	with disk centres,	130 "

and those of 0m 825 diameter—

Wheels	with double spokes,	154 kils.
"	with disk centres,	110 "

the main weight of spoked wheels will therefore be

$$133 + 181 + 154 = \frac{468}{3} = 156 \text{ kils.} \quad . \quad . \qquad (12)$$

that of disk wheels—

$$130 + 110 = \frac{240}{2} = 120 \text{ kils.} \quad . \quad . \qquad (13)$$

that is as 156 : 120, or as 15 : 10 . . . . (14)

Disk wheels beyond all doubt are destined in no long period to supersede those of the present construction: the employment of thin steel tyres will also be rendered capable, by reason of their being tempered without fear of altering in form, they being supported continuously in their interior.

The doubts which have been entertained relatively to the solidity of these tyres need no longer exist, according to the report of the experiments carried out under a commission composed of M. Combes, Laieux, and M. Couche.

This commission has proposed to reduce to one-half the thickness of the tyres, which will enable them to be cast in steel with facility. The conclusions of this report have been adopted by the *Commission centrale des machines à vapeur.* It will be easily perceived how advantageous it becomes to employ steel tyres, which will not quickly wear out, for when their thickness is reduced to a centimetre, (·3937 inches) as far as strength goes they will be equal to iron tyres of 20 millimetres (·7860 inches) in thickness.

According to these conclusions the weight will be, for an axle with wheels attached, of a diameter = 1m 03 = (40·462 inches), and with straight spokes, 815 kilogrammes = (396·6 lbs.); and for the same axle with disk wheels, of the same diameter and steel tyres, 0m 025 = (984·275 inches) in thickness, the total weight would be 645 kilogrammes = (1422·096 lbs.)

On the Amount of Dust lifted by the Wheels of Railway Carriages and Wagons when running.—It will not be contested, after what has been stated, but that disk wheels are far superior in every way to those made with spokes, and, considering the grave objections judiciously raised against the latter, it becomes a desirable matter that these should be transformed into disk wheels, which change may be effected in two different ways, either of which will permit the same wheels as at present existing to partake of the advantages experimentally determined as belonging to disk wheels for railway vehicles. One mode of effecting this change consists in placing on each side or face of the wheel a thin disk of sheet iron or wood; these may be firmly held in place by bolts passing through both at intervals; or, instead of this, it may be preferable to fill the space between the spokes by solid blocks of wood, held to the spokes by means of iron studs. This mode of effecting the jointing is preferable to the use of bolts and nuts, which are usually employed for approximately similar purposes, from the fact that these frequently fall out in traveling, becoming loosened by the centrifugal action of the wheel, and shaken out of their places by the trepidation of the permanent way as well as the train.

The dust raised does not only injure expensive carriages, but it penetrates the axle boxes, and frequently commits serious injury on the journals and their bearings; it is also excessively annoying to passengers.

The Cost and Economy of Disk Wheels.—As to first cost, disk wheels would be about the same as those with spokes, and taking into account the economy in traction resulting from their employment, which, putting it down at minimum, cannot but be of immense importance, as may easily be seen by calculating the number of trips run by each pair of wheels during a given period.

The disk wheels which have served for the above comparison with the spoked ones, were furnished by "*la Societè des forges de la Providence.*"

We have now brought to a close these comparisons between "spoked and disk wheels," and trust that some of our English engineers may ere long bestir themselves toward adopting an improvement which it appears they have been too slow in discovering the advantages of. Disk wheels of cast iron have long been in use upon the American railways, and although it appears likely that these deductions of M. Martin were made with regard to wrought iron disks, still it is very probable that cast iron will ultimately be the material adopted for the purpose. Disk wheels, it is true, have been in use in England for some considerable length of time, the disk being formed of wooden segments under a system patented by Mr. Beattie, of the London and South Western Railway, and the employment of which is largely extending; we have also frequently seen trucks and wagons with wooden disk wheels on the Bristol and Exeter line, but we do not believe that in either case they were adopted for the reason of diminishing resistance to traction, or any of the other advantages that are now shown to belong to them.

We do not confine these remarks to the wheels of carriages, vans, and wagons only—they are equally applicable to those of the locomotive itself, and if the change proposed be ever adopted, it seems very probable that the not unfrequent necessity of mending broken drivers may be quickly obviated. It has long been seen that existing spoked driving wheels are not at all suited for the increasing traffic on our railways; many of them are unable to endure the strain of a modern train. They were all very well and good at their birth; then the weight of the train was in proportion to their rigidity, but now-a-days we see ordinary trains four and five times the weight they were ten or a dozen years ago—still we have the same engines and the same wheels set to do twice or thrice their former amount of work. In a few cases, however, new wheels of larger scantling have recently been introduced. This certainly is anything but fair, and the requirements of the case must be met somehow or other. Let us see how this shall best be done. Not very long ago we were witness to a miserable blunder committed by the locomotive superintendent on a certain English line of railway, and on whom the proceeding reflects anything but credit, for, had the results which were likely to follow been seriously weighed beforehand, we can scarcely venture to believe that such a gross mistake would have been perpetrated. The facts of the case are the following:—The company had a limited number of engines, most of which were made some thirteen or fourteen years ago, at a period when the trains they had to draw were seldom more than one-half the weight and number of what they now are, still the same engines were expected and set to do all this extra amount of work; the consequence was, that from the additional number of journeys which they had to run, say nothing about the amount of slipping of the driving wheels, caused by the heavy load, the tyres were in a comparatively short time, loosened, and worn out—so much so, that the annual expenses of repairs were largely increased by this item, at the same time much loss was suffered by the engine itself having to lay by the time during which the tyres were being turned up afresh, or re-tyred, as the case might be. Thus so much capital was doing nothing. How to obviate these recurrences was the problem to be solved, and in what way was this done? Heavy, that is *thick* tyres had all along been used; but it was thought that if these were increased in thickness, and put on the wheels tighter than before, that the loosening would be avoided, and that the wheels would run much longer without being re-tyred; but we shall presently see if this was practically the result. Tyres of a monstrous thickness were ordered (we regret we cannot state the exact dimensions, having missed our memoranda respecting them); they were bored of little less interior diameter than had been the practice before (with thinner tyres), and put upon wheels about 6 feet in diameter; the consequence was, that after lifting the first wheel out of the cooling cistern, the spokes, straight beforehand, were found, each one, to be curved to no slight amount. This, however, was passed over quite lightly, as if merely nothing of consequence, and so several sets of drivers were similarly served. Some of them were, however, soon set to the crucial test. They had not been running many weeks before the spokes became still more curved, and the wheels loose in the tyres, the heavy weight of which, tumbling about on the wheels when running, smashed them through and through every division between every two adjoining spokes, and several of the spokes were also broken; in fact, many of the wheels, when the tyres were taken off, fairly fell in pieces—and thus the last error was worse than the first. All these alterations made with the view of saving expenditure! Now, we venture to state, that had due consideration been given to the results which were likely to follow from such a proceeding, no such course could have been entered upon by any person understanding what he was about. The results were predicted by others, but no heed was taken; nevertheless it was not long afterwards discovered that the despised prophecy was in reality too true. We have cited this instance at some length, mainly because we think it right these things should be made public, in order that others may be warned from running into such or similar errors. We think it unnecessary to enter at further length on this subject at present, and conclude by recommending urgent attention on the part of British engineers to the advantages resulting from the employment of disk wheels for railway purposes; still it must be remembered that the results which followed from above-named proceedings would have occurred with a wheel of any make, provided the tyre was constructed of sufficient section and put on tight enough to crush the interior, which must have given way in some shape.

Paper on Foundations for Chimneys, Blast Furnaces, Heavy Machinery, &c. By JEREMIAH HEAD, Engineer, Stockton-on-Tees.

From the London Practical Mechanic's Journal, July, 1864.

Large works, such as blast furnaces, rolling mills, &c., are generally sought to be erected, other things being equal, on the banks of a navigable river. The advantage of a private wharf is the reason for preference given to such sites.

The Clyde, Tyne, Wear, Tees, Humber, Thames, Avon, and Mersey, have each of them their banks studded more or less with manufactories. Most of these works have erections requiring sound and immovable foundations, as, for instance, tall chimneys, furnaces, and heavy machinery, and it frequently happens that the banks of rivers afford only the very worst of foundations.

Fig. 1.

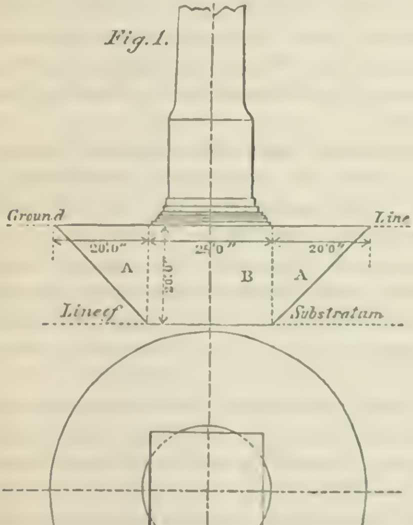
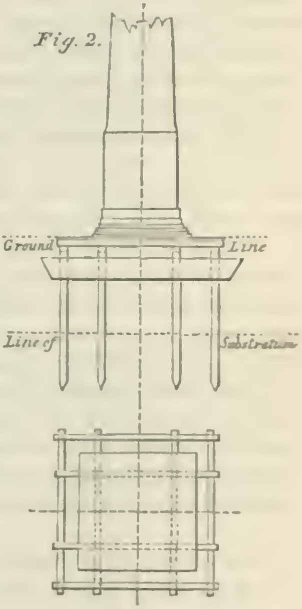


Fig. 2.



Take the Tees for instance, the heart of the Cleveland district, and the banks of which are becoming rapidly disposed of as sites for various iron works. The south bank consists mostly of an extensive flat marsh, in many parts as much as four feet below the level of spring tides, which are kept in check by an artificial bank. A section of the strata gives usually a crust of four or five feet of alluvial clay, in all probability gradually deposited by floods from the river. Below the clay we have from five feet upwards of peat, containing trunks of trees which have known other days. Below that again, down to from twenty to thirty feet, we find a soft bluish silt, incapable of affording a foundation. At twenty to thirty feet there is often either good clay or a hard sand, either of which are sufficient to satisfy the most fastidious engineer.

It is then from this depth, of from twenty to thirty feet, that we must support our fabric.

There are two ways commonly in vogue for obtaining a foundation from this substratum. The one is to excavate down to it and fill up entirely or partly with solid concrete, (see fig. 1,) the other is to pile, (see fig. 2.)

Both these methods are attended with disadvantages. In the former, the ground in the above named locality is, as vulgarly termed, "rotten" to such a degree, that a batter of one to one is necessary to prevent land slips, and should it be in the winter season, even this is insufficient. This is supposing that a foundation of, say not less than twenty-five feet diameter upon the substratum as for a chimney is necessary. If that substratum be twenty feet below the surface, the excavation must be $20 + 20 + 25$ or 65 feet diameter at the top, which gives about $3\frac{1}{2}$ times the solid content of what would be necessary if we could make the excavation twenty-five feet at the top and dig perpendicularly down without batter.

If the excavation is filled up with solid concrete, there is the drawback of the expense of $3\frac{1}{2}$ times as much as is actually necessary, as the overhanging parts, A A, (fig. 1,) take a bearing only on the "rotten" soil, which we suppose at starting is not to be trusted.

And if we build a solid cylinder of concrete, equal in area to the bottom of the excavation, and with perpendicular sides, then soil must be filled in all round, doubling the account for excavation of the parts A A, or in figures making it $3\frac{1}{2}$ less, $1 = 2\frac{1}{2} \times 2 = 5$ times the content of the solid cylinder B, *i.e.* five times more than is necessary.

And yet we cannot get the 25 feet circle upon the solid substratum without all this extra labor upon this system. Another objection is the form of this kind of foundation. According to all mechanical principles the base of any structure should be the broadest part, the work gradually narrowing as it ascends, like a basin with its open side downwards.

But on this plan from the difficulty in excavating in any other way, the foundation has to assume the form of a basin with its open side upwards, and hence, should there be any irregularity of weight, or position of the superstructure, there could hardly be a worse form to resist a tendency to roll. Piling, which is the method of securing a good foundation generally preferred in such cases, is liable to the following objections, viz:—

1st—It is very expensive, and if the piles are required of a greater length than about 35 feet, they are often difficult to obtain at any price.

2d—If the pile heads project above the surface they are liable to decay at the surface line. If the concrete, usually placed about their heads and crown-trees to keep them in position, be upon rotton ground, as in the case assumed, it frequently subsides, tearing itself away from the crown-trees, and leaving the pile heads liable to decay.

If flues, kilns, or stoves pass near them, the crown-trees are apt to get charred or burned, letting down the superstructures. A case of several air stoves, for blast furnaces, being rebuilt from this cause, has lately come under the writer's notice.

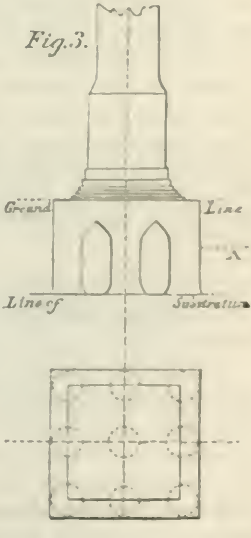
To obviate these difficulties, the writer would submit a plan which he has patented, and believes to be new, having never seen or heard of it before.

Although it is not practicable to dig down twenty feet in "rotten" ground, to obtain a circle of 25 feet diameter, and upwards, without allowing so much batter as to increase the cost of excavation most materially; it has been found by experiment perfectly possible to sink a well of, say 6 to 8 feet diameter, perpendicularly down to that depth, without any fear of land slips.

The circle is so small that the surrounding strata, pressing together, feel the resistance of the arch form, and neutralize one another.

It is proposed, then, to take advantage of this fact, to make the foundations of a series of wells, dug one by one, in order to keep the work perfectly under control, and filled up with concrete or slag walling, *i.e.*, rough rubble walling, formed with lumps of blast furnace slag, a material greatly abounding in the above locality. If a greater depth than 24 feet, or if the nature of the stratum renders it necessary at a less depth, it is recommended to make the wells square, with two planks at each corner placed vertically, each taking a side of the corner; these planks to be strutted at intervals in the depth; they are to be hammered down as the work proceeds, and drawn out as the concrete rises. An immediate stage, with an extra hand upon it, will lift all the soil down to a depth of, say, 12 feet, below which a jack-roll and buckets will be necessary. Near the bottom it is recommended to widen the base of the excavation slightly, in order to incease the footing of the concrete pillar. It is obvious, that by this plan, instead of the foundation being firmest under the centre of the chimney, or other structure, forming a pivot upon which it might tend to work, the concrete pillars may be disposed more at the corners, forming a spreading base which is the desired end to obtain.

Fig. 3.



The platform on which to commence building would, on this system, be formed also of concrete or slag walling, say, five feet thick, and moulded in the following manner:—The wells having been filled up to A, fig. 3, within, say, 6 or 8 feet of the base line of the structure, they would then be widened out, working from a round section into a square one, if a round section has been adopted. The intermediate soil must then be cut into the form of a soffit of a ground arch, and the concrete made up to the ground line in the form shown in fig. 3.

This foundation, besides the advantages of easy execution, wide spread base, and minimum cost, possesses the very great one of being formed only of imperishable materials.

We will now compare the costs of foundations on the three different plans for a chimney, say 500 tons in weight where the substratum was 20 feet deep.

1st. *Solid Concrete Foundation*, 25 feet diameter at bottom, 65 feet at top, and 20 feet deep—(fig. 1.)

Excavation, 1272 cubic yards, at 7½d,	£39	15	0
Cylinder of concrete, 25 feet diameter and 20 feet deep, 363 cubic yards, at 3s. 6d.,	63	10	6
1272—363=909 cubic yards, at 7½d. for filling up round concrete,	28	0	0
Pumping, say,	20	0	0
	£151	5	6

2d. *Pile Foundation*—(fig. 2.)

16 piles, 30 feet long, at 6s. each driver,	£48	0	0
Crown-trees, 260 feet, at 2s,	26	0	0
3 planking overtops, 1056 feet, at 4d,	17	12	0
Excavation, 274 yards, at 7½d,	8	11	3
Concrete, 182 yards, at 3s. 6d.,	31	17	0
Spikes,	0	10	0
Use of pile driver, say,	3	0	0
	£135	10	3

If the under crown-trees alone were whole timbers, and the upper ones half-timbers, 6 inches apart, thus forming the platform and doing away with three inches planking, a better job would be made, but the cost, £144.

3d. *Arched Foundation*—(fig. 3.)

9 wells, 6 feet diameter, each 21 cubic yards, platform 30 feet square and 5 feet deep, say 300 yards of excavation, at 7½d,	£9	7	6
300 yards of concrete or slag walling,	52	10	0
Pumping, say at 3s. 6d.,	18	0	0
Labor for jack-roll, say,	5	0	0
	£84	17	6

It thus appears that the arched system, besides the advantages of better mechanical form and imperishability, would cost in the case assumed little more than half as much as either of the usual methods.

In some cases it may be advisable to carry out the same principle by excavating in deep trenches, either in straight lines or in circles, and filling up and arching over, as explained above. An annular excavation, with, perhaps, a single massive pillar in the middle, is particularly recommended where there is not room for an extended base on pillars, as, for instance, two or more blast furnaces close together.

The excavation while in progress is strutted from side to side to prevent it falling in, and is filled up with concrete or slag walling as it advances.

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, March 8, 1864.]

From the Lond. Practical Mechanic's Journal, April, 1864.

This report was a double one, embracing the month of January as well as that of February—since the annual meeting having been held upon the same day as the last ordinary monthly one of the Executive Committee, the consideration of engineering matters had then been postponed till the next meeting. Of this report the following is an abstract:—

"During the last two months, *i.e.*, from January 1st to February 19th, inclusive, the ordinary visits of inspection have been made; 2 boilers tested by hydraulic pressure, and 307 defects discovered in the boilers examined, 10 of which were dangerous. Details of these defects will be found in the following statement:—

Furnaces out of shape 5; fractures 8 (1 dangerous); blistered plates 3 (1 dangerous); corrosion 45 (5 dangerous); grooving-internal 6; feed apparatus out of order, 1; water-gauges ditto, 45 (2 dangerous); blow out apparatus ditto, 43; fusible plugs ditto, 1; safety-valves ditto 5; pressure-gauges ditto, 13 (1 dangerous); boilers without glass water-gauges, 14; without pressure-gauges 4; without blow-out apparatus 24; without feed back-pressure valves, 83; cases of over pressure, 5; deficiency of water, 2; in all 307.

Of some of the defects enumerated above a few particulars may be given:

Blistered Plates.—A blister, measuring 24 inches by 12 inches, and three-sixteenths of an inch in thickness, has been met with inside an internally-fired boiler, 8 feet in diameter, and working at a pressure of 45 lbs. on the square inch.

Corrosion—Internal.—In two boilers, though only eighteen months old, the plates, which had been seven-sixteenths of an inch thick originally, were found to be eaten half through, while the rivet heads also were attacked. The plates were honey-combed, being affected more severely at the furnace than at the back end of the boiler. The feed-water used was drawn from a well.

In another boiler which was of Lancashire construction, and fed from the Rochdale Canal, the furnace crowns presented a spongy appearance, and channeling had set in at the transverse seams, which is unusual in internal flue tubes; also the rivet heads and angle irons were attacked, as well as the whole boiler generally. Some of the channels were cut half through the plate, while many of the other indentations exceeded this.

Internal corrosion has been successfully arrested, in many cases by the use of carbonate of soda, introduced in small and frequent quantities with the feed-water.

Corrosion—External.—Two wagon boilers were found to be so corroded along the brick-work seating, that holes could be scraped through the plates.

Two Lancashire boilers, set upon side walls, though free from leakage at the seams of rivets, were found on "thorough" examination, to be seriously corroded at the seating, in consequence of water, arising from the discharge of the safety valves, &c., being allowed to fall upon them.

Two other instances occurred to Lancashire boilers, in consequence of their being set upon mid-feather walls. It is always recommended that a portion of this wall should be removed, by way of preparation for a "thorough" examination, otherwise the true condition of the plates cannot be ascertained. In this instance, this recommendation was not complied with in time for the inspector's examination, so that he was unable to see the amount of corrosion going on. But upon this recommendation being subsequently adopted, the plates were found to be so dangerously eaten away, that the owner at once condemned the boiler, and our inspector found it removed on his next visit. In the other case, the plates over the mid-feather, in the neighborhood of the transverse seams, were found to be reduced in places to one-eighth of an inch in thickness.

Four cases of external corrosion occurred at the bottom of the shells of internally-fired tubular boilers, on account of the unequal expansion of the metal, consequent on the imperfect circulation of the water. The constant straining induced by this unequal expansion had produced leakage at the transverse seams, and thus corroded the plates. In such cases caulking is of little use. In many boilers of this class under inspection, the edge of the overlap has been so repeatedly trimmed and recaulked, that the margin beyond the rivets has been at length all cut away. The only radical cure is to maintain the whole of the shell at an equal temperature, and this, in many cases, has been found to be sufficiently accomplished by carrying a return flue under the bottom of the boiler.

Two other cases arose from leakage at the tube ends of multitubular boilers, from which the plates at the bottom of the combustion chambers were nearly eaten through.

Pressure gauges out of order.—One has been met with which only indicated 14 lbs. as the blowing off pressure, while it proved to be as much as 35 lbs. This shows the importance of all boilers being fitted with suitable taps, in order that he inspector may apply his indicator, so as to ascertain the actual pressure, and check the accuracy of the steam-gauge at each of his visits.

Explosions.—An explosion occurred on Wednesday, December 30, 1863, which was not reported in time to take its place in last year's list, so that the number of explosions for the year 1863, which came under the notice of the Association, will now stand as 48; while the number of persons killed and injured will remain unaltered, since, happily, no one was hurt by the explosion just referred to.

The boiler in question, which was not under the care of this Association, was of plain cylindrical egg-ended construction, and externally fired.

The cause of the explosion was simply neglect on the part of the engine-tender, who allowed the boiler to get short of water. In consequence of this the plates over the fire became red-hot, and so weakened, that rupture ensued. Though the rent was confined to the bottom of the shell immediately over the fire, the boiler was blown from its seat and thrown upon the ground, bottom upwards; while another, working alongside, was also dislodged, and thrown to a greater distance than the one that burst.

The engine-tender was prosecuted for neglect, and sentenced to a month's imprisonment with hard labor.

Of No. 47 explosion, which occurred on December 31, 1863, some additional particulars of interest have been obtained since the issue of the last report.

The boiler referred to was No. 1 of a series of three, connected together and working side by side, all of them being of plain cylindrical egg-ended construction, and externally-fired. The furnace end was rent into five or six pieces, and the fragments scattered, some of them being blown to a considerable height, while the remaining portion of the shell was thrown from its original position, and the seating altogether destroyed.

It is of interest to note that the adjoining boiler was also dislodged, and battered in at its side for nearly half its length, since it affords an illustration, by no means the first that has been met with, of the manner in which, what may be termed compound explosions arise, and which it appears spring from externally-fired boilers only,

From the commencement of the present year up to February 19, five explosions have to be reported, from which fifteen lives have been lost, and also twelve persons injured, some of them very severely. Not one of the boilers in question was under the charge of this Association.

These explosions have not all been personally investigated, and when such is the case, the Association is frequently indebted, as in the present instance, to engineers, who, residing in the respective localities of the explosions, and having the opportunity of making examinations, kindly furnish the detailed reports and sketches.

No. 1 Explosion.—The boiler in question worked at a colliery, and was not under the charge of this Association. It was one of a series of two working side by side, both of them being of balloon or haystack shape, 13 feet in diameter, and having their safety-valves loaded to 14 lbs. per square inch, although usually the engine worked with steam at a pressure of 5 lbs. only.

The explosion took place on a Monday morning, the boiler and the engine having been at rest on the previous day. The weather was frosty at the time, and the feed pipe, as was found upon examination after the explosion, became choked with ice. This cut off the supply to the boiler, and it consequently exploded from shortness of water. The boiler was rent into seven pieces, the steam pipes broken up, a portion of the engine-house thrown down, and the top of the chimney-stack damaged. The fragments of the shell and steam pipes, as well as the debris of the building, were scattered to a considerable distance, but although there were one hundred and twenty men in the vicinity of the boiler, and many of the flying bricks struck the roof of an adjacent cottage, in which its residents were sleeping at the time, yet happily no one was injured.

No. 2 Explosion, which resulted in the death of one man, occurred to a boiler not under the inspection of this Association, and employed driving an engine attached in a limestone pit.

It appears that the boiler was 6 years old and one of a series of two, working side by side, and connected together, both of them being of plain cylindrical egg-ended construction, and externally-fired. The length of the one in question was 31 feet 8 inches, the diameter 7 feet and the original thickness of the plates from three-eighths to seven-sixteenths of an inch; while the pressure to which the safety-valve was loaded was 48 lbs. This, however, would be exceeded whenever the steam blew off at all briskly, and, since there was no steam-gauge, the exact pressure would not be known. Excepting the omission of the steam pressure-gauge, which, though too common, should never occur, the boiler was fitted with such mountings as are usual to its class, and might have been safely worked at a pressure of 50 lbs. as long as it was in good condition.

Such, however, was not the case. Leakage had been going on for some time on the top of the boiler, both at the seams of rivets and at the joint of the feed-pipe, which was not riveted to the shell, as it should have been, but merely bolted. The water from this leakage ran down and corroded both sides of the boiler, though it affected the right hand one more especially, the plates of which it reduced in thickness to nearly one-sixteenth of an inch, for a length of about 8 feet horizontally, just at a part concealed from view by the brick-work in which the boiler was set. At this weakened part, which was below water-line, the shell rent in the first instance, and then tore from end to end, opening out nearly flat and separating into three pieces.

All these fragments were thrown from their original position, one of them to a distance of a few feet only, and the other two about 40 yards across a colliery railway. The seating of the boiler was torn up, the engine-house shattered and unroofed, and the debris scattered in every direction. The man who lost his life—though standing on the opposite side of a canal to that on which the explosion occurred, and at a distance of about 40 yards—was struck by a brick shot away by the percussive action of the steam, and died shortly afterwards from the effects of the blow; while the engine-driver and another man in the engine-house escaped comparatively uninjured.

It appears that there was plenty of water in the boiler at the time that it burst, and there is no need of the supposition, that any unusual pressure of steam existed, since the fact of the plates being thinned by corrosion, as they were found to be, is quite sufficient by itself to account for the explosion.

At the inquest, the owner of the boiler, through his solicitor, attempted to prove the engine-man at fault, and show that the explosion was due to excessive pressure of steam, consequent on his negligence, and not to the defective condition of the boiler. The jury, however, returned a verdict of manslaughter against the proprietor, as well as the superintending engineer of the works, both of whom it appears had been apprized, more than once, of the leakage which was going on, and their attention called to the necessity of further examination. The verdict of the jury was, however, shortly after set aside on account of informality.

No. 3 Explosion, from which one person was injured, but happily no one killed, occurred to a boiler not employed in the generation of steam power, but for heating and ventilating a public building. This boiler was not under the inspection of this Association.

It will be seen by the date, (January 11th,) that this explosion took place during the severe frost at the commencement of the year; while two other explosions happened to household boilers, just about the same time, by which one person was killed and three others injured. It was through the frost that these explosions arose. The water in the pipes froze and thus the outlets became choked, and the steam pressure bottled up, so that on the fire being maintained for a sufficient time explosion became inevitable from accumulated pressure. As long as the water in these boilers retains its liquid state its own inlet column forms a natural safety-valve, but since this action is entirely arrested on the occurrence of frost, every boiler employed for heating and ventilating public buildings, as well as those for household purposes, should be fitted with an efficient mechanical safety-valve, which would remain unaffected by changes of temperature. The importance of this with regard to domestic boilers is too apt to be overlooked, but I have known neglect of this precaution to result in fatal consequences in more than one instance.

No. 5 Explosion was of a very serious character, resulting, not only in considerable damage to property, but also in the death of thirteen persons, and in injury, which in some cases was very severe, to ten others. It took place at an iron works, the boilers of which were not under the charge of this Association. The scene of the explosion has been personally visited, and the cause investigated, but, since the coroner's inquiry is still pending, the publication of the particulars is deferred to the next monthly report.

P.S.—Since the above report was closed, which only extends, as stated, to the 19th of February last, another most serious explosion, resulting in the death of ten persons, as well as injury to seventeen others, has occurred at an iron-works to a boiler not under the charge of this Association. The scene of the catastrophe has been personally visited, and the particulars will be given in the monthly report for March to which the explosion belongs.

It will be observed that already twenty-five persons have been killed since the commencement of the year by the six explosions which have occurred.

On Alloys Containing Tungsten.

From the London Mechanics' Magazine, March, 1864.

Caron has instituted a series of experiments by order of the French Minister of War to determine the influence produced by tungsten upon the qualities of bronze, cast iron, and steel. The only French mine from which tungsten is obtained in quantity belongs to M. Dubreuil, and is situated at Puy-les-Viques near St. Leonard, in the department of the Haute Vienne. The wolf-ram is here pulverized and roasted to separate sulphur and arsenic. In this state it could be sold at 2f·60 the kilogramme, but it is better to reduce it to the metallic state by heating with enough carbon to reduce the metals. The crude tungsten contains iron, manganese, and carbon, and is now sold at 3f·75 the kilogramme, a price which will probably be still further reduced. Tungsten as thus prepared was found incapable of forming true alloys with copper, tin, and gun-metal, the latter becoming less homogeneous and less tenacious than ordinary bronze, although harder. The addition of tungsten to cast iron was found on the contrary to increase both the hardness and tenacity in about the same ratio with the quantity of tungsten added. Even a small percentage, not exceeding one per cent. of tungsten, was found to exert a marked influence, the grain of the iron becoming regular, fine and grayish, and the fracture showing great homogeneity. The addition of tungsten, to steel was found always to increase both its hardness and its tenacity. The alloy exhibits a peculiar fracture with a brilliant lustre, so that tungsten steel is easily recognised by a practised eye. Poor steel requires more tungsten than steel of good quality. A good cement steel alloyed with 5 per 100 of tungsten gave a steel of excessive hardness which, however, forged very well, though it required much more force than ordinary steel. After tempering, it acquired a hardness comparable only to that of the hard white cast iron. A gun barrel made of steel, which on analysis proved to contain tungsten, withstood the explosion of a charge 0m.6 in length and weighing 150 grammes, with 5 balls of lead weighing 135 grammes. The author succeeded in obtaining a steel of similar quality by fusing together in an earthen crucible at a very high temperature, 200 grammes of highly carburetted steel, 800 grammes of good iron, and 20 grammes of tungsten. In conclusion, Caron confidently recommends the employment of tungsten to improve the qualities of steel, and shows that with tungsten at 3f·70 the kilogramme, the price of steel would be increased by only 7 or 8 francs the 100 kilogrammes.—*Ann. de Chimie.*

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, No. 207.

(Continued from page 119.)

3. *Discovery of the Pressure of the Air.*—Some years after the publication of the "Mathematical Dialogues," a work "On Motion" appeared, the ingenuity of which so struck Galileo, that he sought the acquaintance of its author, Torricelli. Torricelli* became the pupil of Galileo, but he was not long destined to study under so great a master, for three months after their first acquaintance Galileo died; not, however, before he had imparted to his pupil much of the knowledge acquired during a most active scientific life, and had exhorted him to investigate the cause of several unexplained phenomena, among others that of the limited height to which water rises in a pump.

In making some experiments with a view of arriving at the cause of this latter phenomenon, Torricelli filled a tube, closed at one end, four feet long and a quarter of an inch in diameter, with the heaviest liquid he could find, viz: mercury, and inverted it in a basin of the same liquid; instead of the mercury remaining suspended in the tube, as water would have done, it sank to such an extent that its height above the surface of the mercury in the basin was about 30 inches. Torricelli concluded that this result was produced by the air pressing on the surface of the mercury, but other philosophers preferred to attribute it to Nature's horror of a vacuum.

In 1644, an account of the experiment was sent to Mersennus,† who conceived that there were small hooks in the atmosphere which supported the column of mercury in the tube.

Pascal‡ was not in Paris when Torricelli's experiment was first made known there, and did not hear of it till 1646. He immediately made some experiments on the subject, and published an account of them in 1647, in a treatise entitled "New Experiments concerning the Vacuum," from which we select the following:

A glass syringe with an accurately fitting piston was placed entirely in water, the open end closed by the finger, and the piston raised. An empty space was apparent in the syringe, and the finger was strongly pressed against the glass, but by increasing the magnitude of the empty space, no greater pressure was felt by the finger. When the closed end was opened, water immediately filled the empty space.

A glass tube 46 feet long, open at one end and sealed hermetically at the other, was filled with red wine, and inverted, the open end being placed in a vessel of water. The column of wine sank till it remained at the height of 32 feet§ above the surface of the water. When the tube was inclined, so that no part of it was more than 32 feet above the water, the wine rose and entirely filled the tube.

A syringe was placed in a vertical position, and its open end plunged in mercury; on raising the piston, mercury was observed to follow it, until a height of 2 feet 3 inches had been attained, beyond which, however much the piston might be raised, the mercury refused to ascend.

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* Born, 1608. Died, 1647.

† Born, 1588. Died, 1648.

‡ Born, 1623. Died, 1662.

§ A foot = 1 foot 1·125 inch.

From these and several other experiments, Pascal declares it to be his belief that the space above the mercury in Torricelli's experiment is not full of either ordinary air or that air which some philosophers assert is inclosed in the pores of all bodies, or of air remaining by chance between the liquid and the glass, nor does it contain vapor of mercury, or that subtle air said by some to be mixed with ordinary air. It is veritably void and destitute of all matter.

These views were combated by Father Noel, a Jesuit, who believed that the space above the mercury was only an apparent void. Noel affirmed that just as that which we call blood is a mixture of bile, phlegm, "melancolic," and of blood, so that which we call air is a mixture of fire, air, water, and earth. The fire and air, being more subtle than the water and earth, can penetrate glass, and the space which Pascal had stated to be absolutely vacuous he believed to be filled with a pure air, compelled to separate from its grosser part by the weight of the column of mercury, which dragged it through the pores of the glass. These objections were answered by Pascal, and their fallacy proved beyond dispute.

In the "New Experiments concerning the Vacuum," Pascal had given no actual proof that the suspension of the mercury was due to the pressure of the air, but soon after the publication of that treatise he thought of an experiment which would settle the matter definitely one way or the other; but as it was impossible to try it in Paris, he wrote to his friend Perier, who resided in Auvergne, and begged him to make the experiment.

In his letter to Perier, which was written on November 15, 1647,* he says he cannot believe that Nature, which is not animate or sensible, can be susceptible of horror, because passions presuppose a soul capable of sharing them. He believes that all the phenomena attributed to Nature's horror of a vacuum, are caused by the pressure of the air, and has devised an experiment which must decide the matter. "This is to make the ordinary experiment of a vacuum, several times in the same day, in the same tube, with the same quicksilver, sometimes at the bottom, and sometimes on the summit of a mountain, elevated at least 500 or 600 toises,† in order to prove if the height of the quicksilver suspended in the tube will be equal or different in these two situations. You undoubtedly already see that this experiment is decisive of the question, and that if it happens that the height of the quicksilver is less on the summit than at the bottom of the mountain (as I have many reasons for believing, although all those who have thought on this matter are against this opinion), it will follow necessarily that the weight and pressure of the air is the only cause of this suspension of quicksilver, and not the horror of a vacuum, since it is certain that there is more air which presses on the foot of the mountain than on its summit."

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* See "Récit de la Grande Experience de l'Equilibre des Liqueurs."

† A toise = 6·39459 feet.

Pascal suggested that the experiment should be made on the summit of the Puy de Dôme, a mountain in Auvergne; but Perier was at Moulins when the letter reached him, and was unable to make the trial till the following September, more than eleven months after the receipt of the letter.

On September 19, 1648, Perier provided himself with six livres of well purified mercury, and two glass tubes, exactly similar in every respect, 4 feet long, and closed at one end. The tubes were filled with mercury, and inverted in the same vessel, at the base of the Puy de Dôme: the height of the column in each tube was found to be absolutely the same, viz: 26 inches 3½ lines[*] above the level of mercury in the vessel. One of the tubes was left at the bottom of the mountain, the height of the mercury within it being marked, and a man appointed to watch if it varied. Perier carried the other tube and part of the same mercury to the summit of the Puy elevated about 500 toises above its base, and on repeating the experiment, the height of the column of mercury was found to be only 23 inches 2 lines; "ce qui," writes Perier, "nous ravit tous d'admiration et d'étonnement." The experiment was repeated five separate times in different parts of the summit of the mountain, with the same result.

During the descent the tube was filled and inverted; at Lafon de l'Arbre: the height of the mercury was 25 inches. When the experiment was repeated at the bottom of the mountain in the same tube, the mercury was found to remain suspended at the original height of 26 inches 3½ lines, and the column in the tube which had been left there also stood at that height. The following day Perier repeated the experiment at the top and bottom of Notre Dame de Clermont, and found a difference in the height of the mercury amounting to 2 lines.

The different heights of the column of mercury at the various elevations at which Perier had made the experiment were as follows:

At Clermont,	26	inches	3½	lines.
7 toises above Clermont,	26	"	3	"
27 " " "	26	"	1	"
About 150[†] "	25	"	0	"
" 500 "	23	"	2	"

These results were communicated to Pascal in a letter dated September 22, 1648, and they fully confirmed the views which he had previously held.

The Puy de Dôme experiment proved indubitably that the suspension of the mercury in Torricelli's experiment was due to the pressure of the air, and not to Nature's horror of a vacuum. Hitherto, not only Torricelli's experiment, but all the phenomena which were at that time attributed to Nature's horror of a vacuum, could be explained equally well by this theory, or by that of the pressure of the air; and when Torricelli first made known his experiment, men preferred to explain it by Aristotle's theory, which had been received for so many centuries, rather than to accept a new explanation, of the truth of which they had no absolute proof. But the Puy de Dôme experiment supplied the absolute proof which was wanting, for (as Pascal says in his first letter to Perier) we know that more air presses on the bottom of a mountain than on its summit, but no one can affirm that Nature has a greater horror of a vacuum at the level of the sea than she has on the summit of a mountain; if, therefore, the column of mercury is found to sink the higher the tube is carried, proof is at once afforded of which theory is to be accepted, as truly explaining not only Torricelli's experiment, but the whole class of phenomena attributed to Nature's horror of a vacuum.

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[*] 12 lines = 1 inch.

[†] From the height at which the mercury stood Lafon must be at least 200 toises above Clermont.

This was one of the first, and assuredly one of the greatest applications of Bacon's "Instances of the Cross,"* and I do not know a more perfect example of the way in which this "help of the understanding" was intended to be used for the interpretation of Nature.

For more than 2000 years men had been traveling along the road pointed out to them by Aristotle; but Torricelli, although he kept to it for some distance, at a certain point constructed a new road, branching off from the other, which he conceived would lead travelers more surely whither they desired to go. But there was no direction post, and when travelers came to the division they knew not whether to still keep to the road of Aristotle, or to turn aside into the branch road of Torricelli, and the generality preferred to continue their journey by the beaten well-known track. It was reserved for Pascal to erect the cross at the juncture of the two roads, to point out the right way to the traveler, and it still keeps its place, and the direction which he graved thereon still remains, and ever will remain, a monument to a glorious intellect.

Some time after hearing of the success of the Puy de Dôme experiment, Pascal requested Perier to observe the different heights at which the column of mercury stood at different times, and during various changes of weather, in the same locality. Perier made daily observation at Clermont, from the beginning of 1649 to the last day of March, 1651. A friend to whom he wrote in Paris made observations from August 1, 1649, to the last day of March, 1651; and MM. des Cartes and Chanut made observations in Stockholm from October 21, 1649, to September 24, 1650.

The following results were obtained:

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* Extract from the thirty-sixth aphorism of the Second Book of the "Novum Organum," translated by J. Devey:—"In the fourteenth rank of prerogative instances we will place the Instances of the Cross, borrowing our metaphor from the crosses erected where two roads meet, to point out the different directions. We are wont also to call them decisive and judicial instances, and in some cases instances of the oracle and of command. Their nature is as follows:—When in investigating any nature the understanding is, as it were balanced, and uncertain to which of two or more natures the cause of the required nature should be assigned, on account of the frequent and usual occurrence of several natures, the Instances of the Cross show that the union of one nature with the required nature is firm and indissoluble, whilst that of the other is unsteady and separable; by which means the question is decided, and the first is received as the cause, whilst the other is dismissed and rejected "

At Clermont (about 400 toises above Paris) the greatest height of the column of mercury was 26 inches 11½ lines (February 14, 1651). The least height was 25 inches 8 lines (October 5, 1649).

In Paris the greatest height was 28 inches 7 lines (November 3 and 5, 1649). The least height was 27 inches 3½ lines (October 4, 1649).

In Stockholm the greatest height was 28 inches 7 lines (December 8, 1649). The least height was 26 inches 4¾ lines (May 6, 1650).

Perier noticed the fact that sudden changes in the height of the column often take place; thus, on December 6, 1649, the mercury stood at 27 inches 5 lines, while on December 8 it had risen to 28 inches 7 lines.

It was Pascal's intention to thoroughly explain in a large treatise the principal questions relating to the equilibrium of fluids, the vacuum, and the pressure of the air, but he abandoned this idea, and contented himself with writing two short treatises "On the Equilibrium of Fluids," and "On the Weight of the Mass of Air;" these were found among his papers, and were published in 1663, one year after the death of their illustrious author.

The proofs of the assertions made in the treatise "On the Weight of the Mass of Air," are mainly taken from experiments described in the treatise "On the Equilibrium of Fluids;" the air is considered to act in every way as a liquid, and that which he has found to hold good with water, he considers to be equally true for air. In the first chapter of the treatise "On the Weight of the Mass of Air," Pascal proves that air possesses weight, and that it presses upon all bodies which it surrounds.

In a heap of wool twenty or thirty toises high, the wool at the bottom is more compressed than that in the middle or near the top of the heap, because it is pressed down by a greater weight of wool above it; similarly the air at the bottom of valleys is more compressed than that on the summit of mountains, because it has a greater weight of air above it. If a handful of wool is removed from the bottom of the heap, and placed in the middle or near the top of it it will enlarge itself, because it will have a less weight of wool to support than it had before, so, also, if we take air from a valley and carry it to the summit of a mountain, it will enlarge itself and become in a similar state of density to the air surrounding it, because it will have a less weight of air to support than it had in the valley; this may be proved by partially filling a bladder with air from a deep valley and carrying it to the top of a high mountain—the bladder will become more and more puffed out the higher it is carried.

In the second chapter Pascal proves that the weight of the air produces all those effects hitherto attributed to Nature's horror of a vacuum. This chapter is divided into two sections. In the first Pascal mentions the principal phenomena which had up to that time been attributed to Nature's horror of a vacuum, and in the second he shows how these phenomena are caused by the pressure of the air.

Among the effects attributed to Nature's horror of a vacuum were the following:—

Bellows, all the openings of which are closed, are difficult to open, and the piston of a closed syringe is raised with difficulty.

Two bodies with accurately-ground and polished faces when placed in contact are not readily separated.

When a syringe or pump is placed in water, and the piston raised, water follows it, in order to prevent the formation of a vacuum which would otherwise take place.

When tow is kindled in a plate containing water, and a glass inverted over it, in proportion as the tow is extinguished the water rises into the glass, because the air which was rarefied becomes more and more condensed, and attracts the water with it to fill the place which it quits in contracting, just in the same way that the piston of a syringe draws water after it to fill the space which would otherwise be a vacuum. Cupping-glasses act in a like manner.

A bottle or short tube closed at one end, if filled with water and inverted in a vessel of water, remains filled.

If we open the tap of a tun of wine not a drop of wine escapes until we remove the vent-peg.

The action of an ordinary siphon is due to Nature's horror of a vacuum.

Pascal next adduces proofs that these effects are due to the pressure of the air.

We cannot open closed bellows without exerting a good deal of force, because in opening them we must elevate the whole mass of air resting on them; but if we make a hole in any part of the bellows the air enters, and we have no longer to raise the air above them.

When two well polished bodies are placed in contact, the lower one will remain attached to the other, because the air presses on it below and not above, and as there is no access for the air between the polished surfaces of the bodies, it presses only the upper surface of the upper body, and on the lower surface of the lower body.

Water rises in pumps and syringes placed in a vessel of water when the piston is elevated, because the air presses on all parts of the water in the vessel, except under the piston where the air has no access; as we raise the piston the water follows in order to counterbalance the weight of air which presses on the water outside the pump.

For the same reason water remains suspended in bottles and short tubes closed at one end, which have previously been filled with water and inverted in a vessel of water.

Cupping-glasses attract the flesh, because the air within them is rarefied by fire, and the outward air presses on all parts of the body, except on that part under the cupping-glass, consequently the flesh is caused to swell within the glass.

The air causes water to rise in siphons because it presses on the water of the vessels into which the limbs of the siphon dip, and not on the water in the interior of the siphon.

In the third chapter Pascal proves that the weight of the air is limited, and, therefore, the effects which it produces are limited.

When the sucker of a pump is raised, water follows it to a height of thirty-one feet, beyond which it refuses to ascend. Mercury refuses to ascend above two feet three inches, and oil above thirty-four feet, the weights are in all cases the same, that is to say, a column of mercury two feet three inches high weighs as much as a column of water 31 feet high, or a column of oil 34 feet high.

When we attempt to open closed bellows, the sides of which are, say a foot diameter, we must, before we can open them, exercise an amount of force capable of raising a column of water 31 feet high and a foot diameter. So also if we have two polished bodies, the diameter of the applied surfaces of which is an inch, we must, in order to separate them, apply a force capable of lifting a column of water an inch diameter and 31 feet high.

In the fourth chapter Pascal asserts that the weight of the mass of air varies according to the vapors which are present in it, and that, therefore, the effects which it produces vary; as an example of this. he mentions the fact that the air will sometimes support a column of water 31½ feet high, and at other times only 30¾ feet high.

In the fifth chapter, the weight of the mass of air is affirmed to be greater in valleys than on the summits of mountains, and the effects produced by it in valleys to be therefore greater than those produced on mountains.

At a height of 500 or 600 toises, a suspended column of water is 4 feet shorter than at the level of the sea. Moreover, two polished bodies which have been applied to each other are separated with greater ease on the summit of a mountain than at its base.

The mercurial column falls about 1 line for every 10 toises of height.

In the sixth chapter Pascal asserts that as the effects produced by the weight of air increase or diminsh, as it increases or diminishes they will entirely disappear above the air, or in a place where there is no air.

As we find the weight of a column of mercury becomes less and less the higher we ascend, we have a right to conclude that the mercury would entirely fall if we could go above the air, or in a place where there is no air; but as this is impossible, we may try the same experiment by completely removing the air from the open orifice of the mercury tube; in order to do this, Pascal connected the open end of one barometer tube with the upper part of a second tube, filled them both with mercury, and placed the open orifice of the lower tube in mercury, the mercury in the upper tube immediately left it, because no air was present to counterbalance its weight.

According to a calculation, which is given in full in chapter 9 of this treatise, Pascal estimates the weight of the entire mass of air which surrounds the earth at 8,283,889,440,000,000,000 livres.*

Among those papers which were found after the death of Pascal there was one containing a number of tables, giving the results of experiments made to determine the weight necessary to separate two polished surfaces from each other, the diameter of the applied faces being respectively 1 foot, 6 inches, 1 inch, and 6 lines.

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* A livre = 1 lb. 1 oz. 10¼ dr. Avoirdupois.

The experiments were made in Paris, about 31·5 toises above the level of the sea. At Clermont, about 400 toises above Paris; at Lafon, 600 toises above Paris; and on the summit of the Puy de Dôme, 900 toises above Paris. The following table represents the weights in livres necessary to separate two polished bodies 1 foot in diameter:—

	Greatest weight necessary.	Medium.	Least.	Difference.
In Paris, . .	1808	1761	1714	94
At Clermont, .	1675	1628	1581	94
At Lafon, . .	1579	1532	1485	94
Summit of Puy, .	1483	1436	1389	94

(To be Continued.)

Refrigerating and Ice-Making Machines.

From the Practical Mechanic's Journal, November, 1863.

(Continued from page 114.)

Passage from the Refrigerator to the Boiler.—The power of the apparatus is proportional to the latent heat of the volatile liquid and to the weight of vapor which forms per hour in the refrigerator. This weight of vapor is only dependent on two things; on the form of the refrigerator, and on the difference which exists between the general elastic force existing in its interior, and the maximum elastic force due to this vapor, at the temperature of the liquid which wets the interior surfaces. In fact, if the interior were saturated with vapor no new vapor could be formed and no cold produced. If, on the contrary, the interior were maintained without vapor, that is to say, in the state of a perfect vacuum, the weight of vapor, formed per hour, would be at its maximum, and the production of cold would also attain its maximum. It is necessary then to exhaust this vapor, which is no sooner formed than it becomes an obstacle, the space which it encumbers must be freed, so as to restore it incessantly to a state of perfect vacuum, or at least as nearly so as possible. There are different means for this, but incontestably the most advantageous is that which can be employed here, that is, to exhibit a body which can rapidly condense it by a dissolving affinity, and which can give it off again with the same rapidity by a sufficient increase of temperature. The boiler is so arranged that the ammoniacal solution, at its lower part, is very weak; a pipe furnished with a stopcock is placed there to withdraw a certain volume of it, which is fixed by the degree to which the cock is opened; this tube, made of iron, and 20 or 30 metres long, is coiled twice at different distances into two helical worms, which are surrounded with cooling liquids. Thus, the liquid contained in the tube, starting from the boiler at 130° and cooled to about 20° to 25° C., reaches the top of the absorbing reservoir to fall as rain into its interior. It is this continual rain of impoverished liquid which becomes the power capable of incessantly maintaining and renewing the vacuum in the interior of the refrigerator. For this purpose a large pipe, some metres long, connects the top of the refrigerator with the top of the absorbing reservoir. As soon as the cock which regulates this communication is opened, the ammoniacal vapors of the refrigerator flow into the midst of the rain of weak liquor, are there condensed by absorption, and reconstitute a rich liquor which falls to the bottom of the reservoir. The heat which is here disengaged is removed by the coils of the worm pipe through which cold water flows; it only remains to take up this rich liquor, and re-introduce it into the boiler, so as to make up for the loss of ammonia which takes place every instant, or rather to restore all which has left it, and thus to close this long circulation, in which change of form and state takes place, but no loss or gain of matter.

It is a lift and force pump of a very peculiar construction, and well adapted to the effect it is designed to produce, which is arranged to accomplish this last part of the circulation. It draws from the bottom of the absorbing reservoir the liquid which has been enriched as fast as it forms, and then causes it to enter a special vessel designed to receive it; it then forces it through a long tube where it is re-heated, to arrive at last at the top of the cascade of which we have previously spoken, and which constitutes the top part of the boiler. This liquid, although re-heated in its course, is far from being at 130° C.: its presence then produces some condensation, the effect of which is no doubt favorable to the rectification of the hydrated vapors of ammonia which exist at this point.

We cannot close this summary description of the apparatus without remarking that we have been obliged to pass over a great number of details, adjustments, and ingenious arrangements, which perhaps prove even more than the general design, the genius, and resources of the inventor. Let us now endeavor to explain to what the economic power of the apparatus extends, and its limits.

This discussion rests on but a small number of data, namely:—On the latent heat of vapor of the ammoniacal liquor, and of the more or less dilute solutions of ammonia. Davy has already drawn up a table of the quantity of ammonia contained in solutions of different densities: this table, which is based on only two experiments, is re-produced in all the chemical books; it is desirable that it should be again taken in hand and extended to different temperatures. In applying it to the practical observations of M. Carré, we are led to believe that in the actual state of things, one kilogramme of the weak solution which enters the absorbing reservoir cold, can there take up 50 grammes of ammonia to become the rich ammonia which is re-introduced into the boiler.

Our fellow-worker, M. Regnault, has been good enough to give to the Commission a proof, extracted from his great work on vapors, which is now in the press. We there find a complete table of the tensions of ammonia solution between the temperatures of 40° and 100°, for the lower temperatures, which are of the most importance here. These tensions are—

Temperatures, . . .	20°	30°	40° C.
Tensions in atmosphere, . .	1·84	1·16	0·70

To apply these numbers to the slightly dilute solution of the refrigerator, we must take account of the diminution which they have to undergo. Estimating this at one-fourth, we get the following result—

Temperature of refrigerator, . .	20°	30°	40° C.
Tension in atmospheres, . . .	1·4	0·9	0·5

which approaches closely to the practical observations of M. Carré.

Lastly, according to the researches of MM. Favre & Silbermann, (Annales de Chimie, t. xxxvii., 1853,) we may value at about 500 units, the latent heat of ammoniacal gas absorbed by a mass of water large enough to form a dilute solution. We admit it as probable, that this number may be applicable to ammonia containing very little water.

It results from these data, that to construct an apparatus whose power shall be, say, 100,000 units of heat an hour, it will be necessary to vaporize 200 kilogrammes of ammonia in the refrigerator, and thus, at the same time 200 kilogrammes must be condensed in the liquifier, and absorbed or condensed a second time in the absorbing reservoir. The 100,00 units of heat, or very nearly, re-appear then in these two members of the apparatus, where they must be taken up and carried off by the cooling water.

Admitting that the temperature of this cooling water only rises 10° in this operation, we see that the expenditure of it will be 20,000 kilogrammes, or 20 cubic metres an hour; that is, 10 cubic metres to cool the liquifier, and 10 cubic metres to cool the absorbing reservoir.

We do not speak of the necessary consumption of fuel in the boiler. In effective result, this should also be equivalent to 100,000 heat units an hour, but there are unavoidable losses, which are very variable.

In a word, the four changes of state, although operating under different conditions, should be accompanied by the same, or nearly the same, phenomena, as far as regards the qnantities of heat concerned.

The boiler and the refrigerator acting by evaporation, take up the same quantity of heat, one from the fire, and the other from the liquid which it cools. The liquifier and the absorbing reservoir acting by liquefaction, should disengage the same quantity of heat, which they must get rid of by continually changed cooling liquids. The mechanical work of the lift and force-pump should also be approximately valued.

Since 200 kilogrammes of vapor are produced an hour in the refrigerator, 4000 kilogrammes of weak solution are required to absorb them, for each kilogramme only absorbs 50 grammes, or one-twentieth of its weight. The result then will be 4200 kilogrammes of strong solution. The work consumed in re-introducing this into the boiler, the necessary pressure being estimated at 10 atmospheres, will consequently be 420,000 kilogrammetres, or about two horse power, to which must be added about one-tenth for the work consumed in suction; but in this the considerable loss of power in working the pump by the disengagement of elastic fluids is neglected.

As to the degree or intensity of cold which the apparatus can produce, it depends almost exclusively on the phenomena which take place in the absorbing reservoir, because there, in fact, exists the determining cause of the rapid formation of vapor in the refrigerator. If, on the one hand, the liquid which gives off this vapor were pure ammonia, free from water; and if, on the other, the weak liquor which comes from the boiler into the absorbing reservoir were pure water, free from ammoniacial gas, there can be no doubt that the refrigerator would easily fall to 50° or 60° C., below zero.

But in reality, the liquid in the refrigerator contains a certain proportion of water, and the weak liquor which enters the absorbing reservoir contains a very perceptible quantity of ammonia. These two causes join in relaxing the absorption of the vapor, and consequently in preventing the degree of cold descending so low in the thermometric scale. Hence there is room for study in making the boiler yield a liquor still poorer in ammonia, and the liquifier a liquid more completely deprived of water.

This ultimate limit to the economic power of the apparatus always depends on still another circumstance: it varies necessarily with the temperature of the air, and consequently with seasons and climates. Let us suppose, for instance, that the refrigerator *works in space*, that is to say, without making ice or cooling a liquid; in a word, without producing *useful effect*, it will arrive nevertheless at a certain limit of cold, which will be its extreme limit, say 50° below zero. Suppose that in this experiment the air has a temperature of 10° C., this gives it an excess of 60° over the refrigerator. Once arrived at this limit, after working for a longer or shorter time, during which the refrigerator might be observed gaining progressively in cold, very quickly at first, and more gently afterwards, it may be asked, How is it possible to maintain this state of things? May we draw the fire from the boiler, stop the pump, and in fact set the apparatus at rest, without the refrigerator becoming warm? Assuredly not. On the contrary, it is indispensable to continue working at full speed. Its entire power is then lost, in the sense of its not being utilized; but it is not lost in reality, for it is wholly employed in maintaining the refrigerator in equilibrium against the inroads of heat from without. We can stop the motion of a machine, but it is not given to us to stop the motion of heat. Whatever precautions are taken to protect the refrigerator, heat always penetrates to it, although the rate at which it does so may be more or less reduced. The number of heat units which thus reaches the refrigerator in a given time, all other things being the same, is nearly proportional to the extent of surface which it presents, more or less directly, to the air, and to the excess of the temperature of the air over that of the refrigerator.

Consequently, if the same apparatus is submitted to a second test in space, with the air at 30° C., instead of 10°, it can never, work as it will, cause the refrigerator to descend to 50° below zero, but will only make it fall to 30° below zero, because the excess of the temperature of the air over the refrigerator will be again 60°, as it was in the first test.

The preceding considerations also permit the inference, that a refrigerator designed for making ice will be much more favorably circumstanced for descending to very low temperatures than one designed for merely *cooling* liquids, because in general the latter will offer much greater surfaces than the former to the access of heat from without.

Such are the principal causes which vary the extreme limit of cold to which the refrigerator can fall, and which vary the economical power of the apparatus in the same sense.

It results from this, that the weight of vapor which forms in an hour in the refrigerator of a given apparatus should be considered as nearly a constant quantity, but composed of two parts, which do different duties. The first produces useful effect; the power of the second is lost. This latter, without ever becoming nil, remains very small when the refrigerator, to perform the effect required is at a temperature considerably removed from its extreme limit; but it increases rather quickly, and always with a detrimental effect, whenever the refrigerator has to work at a very low temperature; and at last it absorbs the whole or nearly the whole, of the useful effect, if the refrigerator has to work at a temperature very near its extreme limit.

These variable losses depend on co-efficients, to which experience will soon give values sufficiently accurate to estimate the useful effect, according to the nature of the work to be done, and the knowledge of the exterior temperatures to which the apparatus must be exposed.

M. Carré has carefully studied all the theoretical principles which should guide him in the construction of his apparatus; he has aided himself in this way by studying with great sagacity all the new questions which bear on the original problem, and he has ultimately arrived at solutions which have the merit of being at the same time highly ingenious and very practical.

"The Commission is of opinion, that the apparatus of M. Carré is capable of rendering real service, and proposes to the Academy to admit the description of it into the 'Recueil des Savants étrangers.'"

The conclusions of this Report were adopted by the Academy.

Poisoning of Water by Lead Pipes.

From the London Mechanic's Magazine, May, 1864.

Professor H. Dussance, of New York, has recently made a series of experiments on the action of several different kinds of water on lead, under various conditions. The lead was subjected to the action for twenty days, and the experimenter draws the following conclusions:—1. That distilled water has no action whatever on lead by three days of contact; after that time this dissolving action begins. 2. That the lead is dissolved by distilled water in proportion increasing every day; the distilled water exposed to the open air dissolves more of this metal than distilled water in close vessels, or than distilled water deprived of air and gas. 3. That creek water, containing small proportions of lime, has no action on lead. 4. That distilled water, containing 1-3500th of a salt in solution, prevents the dissolving action of the water on lead. 5. That water dissolves lead till the saturating power of the acid is exhausted. 6. That, in ferruginous water, all of the iron is precipitated by lead; then lead pipe must not be used to convey mineral waters. This fact has never been noticed before. To render these facts more interesting, another series of experiments must be made to ascertain the quantities of lead dissolved daily in the water, and what compound it forms, and to see if the action will be the same in the lead pipes.

Among all the dangers arising from carelessness, the public may not be aware, that there is great danger in the careless use of even hydrant water. That water, impregnated with any preparation of lead, cannot be safely used for culinary purposes, even in cases where the lead is present in too small a proportion to produce any immediate injury, is well known. Lead, says Professor Aikin, of New York, like some other poisonous agents, when taken in minute successive doses, will remain in the system, apparently inactive, until a certain amount accumulates; then, suddenly, a series of alarming symptoms will supervene, often resulting in death. An occurrence of this kind was recently brought to my notice by one of my professional friends, who was called upon to prescribe for several members of the same family, all apparently suffering from the peculiar action of lead. In one of the cases the result was fatal—the others, after much suffering, slowly recovering. A portion of the water supposed to have been instrumental in producing the difficulty was brought to me for examination, and carefully analyzed, and found to contain lead in the proportion of 0·028 gr. to the gallon of water. This very minute quantity, very little exceeding the 0·001 gr. to an ordinary tumbler full of water, taken at intervals, somewhat regularly, seems to have accumulated until the poisonous action of the metal was developed. The whole difficulty seems to have arisen from the occasional use of water taken from a hydrant stop that was not in regular and frequent use; the water in a portion of the lead pipe being thus allowed long continued contact with the metal in the immediate vicinity of the stop. The simple precaution hereby indicated, and one that cannot be neglected with safety, is to allow the first discharge of water from any hydrant stop to run to waste, and not to collect any for culinary purposes until we are sure that all the previously stagnant water in the lead pipe has wholly escaped. And it would be better, in general, to resort to the yard hydrant for water required for drinking or preparing food—the lesser length of lead pipe there in use diminishing the danger. But even there the water first discharged in the morning should be thrown away.

Electro-Magnet Induction Machines for Telegraphy.

From the London Chemical News, No. 223.

A new induction machine is now to be seen in operation which produces a constant stream of electricity, and can be made to produce it of any tension or quantity that may be required. These are valuable properties, which doubtless will be appreciated by all those who are obliged to use galvanic batteries of high tension. Many attempts have been made to use induced electricity for telegraphy, but as these attempts have been generally made with machines similar to Rhumkorf's induction coil, they have failed, for the following reasons:—The electricity is in impulses, and alternately in reverse directions. It is in to small quantity, and too great tension; so much so, that it is said tha

Mr. Whitehouse's five-feet coil destroyed the Atlantic cable; but the authorities are not agreed on the point. However, it is certain that such machines are not suitable for telegraphy generally, even if one impulse be thrown out, so that all the impulses which are taken may be in the same direction; for there is then no approach to a battery current. This has been done, but in all cases the most suitable impulse has been rejected, namely, the slow one, or making impulse, and the breaking impulse taken, which is of greater tension but shorter duration; of course, the absolute quantity of force in each is the same, and it will be clear that the former, approaching more nearly to a current, would be most suitable. In some cases the two reverse impulses are used; but these can only be used with one telegraphing arrangement, and that a slow one.

What has been wanted, and which is now obtained, is as near an approach to a battery current as may be, and of any tension required, without multiplying the number of battery cells used. Any machine to be of real use must not only possess these properties, but must be perfectly self-acting. The machine here spoken of possesses all these properties. Its current is perfectly continuous for all practical purposes, for no intermittance can be detected. It holds the needle perfectly steady, rings an electro-magnet bell, produces electrolysis, and comports itself in every other way like a battery current, even producing no spark unless the poles be brought into absolute contact. The electricity which is used to excite the electro-magnets in the coils is at the same time made use of as magnetism in the electro-magnets to drive the battery break and commutator; so that the motive power, which makes this machine self-acting, is obtained without cost, because to produce induction the magnets in the coils must necessarily be excited whether the magnetism so induced be otherwise used or not.

Two series of induction coils are used in this machine, which are so arranged that one series is being magnetized nearly at the same time that the magnetism is subsiding in the other, so that the two consequent induced impulses as it were overlap each other; and though these induced impulses are in opposite directions, the spools are so arranged that in the general induction circuit they flow in the same direction, thus making a compound impulse of longer duration composed of the two opposite inductions, and so blending the more intense with the less, the result being a sort of undulation of force. These compound impulses flow in opposite directions, and to make them a continuous flow they are taken up by a commutator which reverses its contacts as they reverse, and thus turns them all in one direction, producing a slightly undulating but continuous flow. This seems to be the form of motion that all known forces take. The machine appears to be made for quantity, the inner coils being of No. 12 wire, and the outer of No. 18. Some electricians who have examined it, have expressed a fear that it might injure the insulation of gutta perchaed wire, as this fault has been attributed to ordinary induction coils. Of course, this objection does not apply to this machine any more than to an ordinary battery; for to whatever purpose it may be applied its tension and quantity will be adjusted to the tension and quantity of the number of battery cells ordinarily required to perform that work; nevertheless, the machine is being subjected to continuous tests to put the thing beyond doubt. A gutta-perchaed wire is punctured and then immersed in water, and the leakage tested, then the full current sent through it for hours, but as yet no perceptible variation has been noticed.

Tubular Boilers.

From the London Mechanics' Magazine, July, 1861.

There is not a question connected with the steam engine or the employment of steam power which has not at one time or another been made the subject matter for bitter discussion. The value of the heating surface of tube flues, in particular, stands forth prominently, as one of the most fertile soils for the production of this kind of argument that has yet been cultivated. We question if two men of science who have given any thought to the matter, can be found at this moment who will agree even on the conditions under which the tubular boiler can be worked to the most advantage. As to its comparative abstract value, it seems all but hopeless to expect an unanimity of opinion. Very many and very elaborate experiments have been conducted from time to time, but, regarded in the proper light, these experiments impart very little information. As a rule, they have been two-fold in their object. In the first place, they have been instituted to determine the value of flue surface as compared with fire-box surface in the same boiler; and, secondly, they have now and again been employed to determine the value of the tubular as compared with the Cornish or the externally fired boiler. As to the first, it is somewhat remarkable that there are no recorded instances of experiments conducted under other than abnormal conditions, that is to say, the generator has never been employed as generators of the class would be in actual work. As to the second, we are at a loss to find an instance wherein precisely the same coals, the same stack and draft, and the same water supply were employed in each case, and it is yet more difficult still, to find an instance wherein all parties were not previously impressed with the superiority of one or other class of boiler over that with which it has been compared. In spite of the utmost conscientiousness on the part of the experimentalist, such a bias is certain to produce results on which it is difficult to place implicit reliance.

The truth of the matter is this: apart from its economical value, the tubular boiler possesses certain constructive advantages which have brought it into high favor. No other practical generator—by which we mean one moderately cheap and not likely soon to get out of order—will bear a moment's comparison with the tubular in its powers of making steam, weight for weight, and size for size. Lighter boilers may be found of equal power, but they are inevitably larger; smaller boilers may be found, but they are sure to be heavier. And, beside all this, the shape of the tubular boiler of the best type is, above all others, that best adapted for a locomotive carriage supported on more than one pair of wheels, and intended to travel at high speeds. This fact in itself is enough to render its popularity for railway purposes a thing which needs no further explanation. Again, for marine purposes, nothing more convenient than the return flue system exists, and without tubes it is not easy to carry out this principle of construction with any success.

Every tube flue has three purposes to fulfil at least. Occasionally it may have to perform a fourth, to which we shall refer further on. The first is to permit the passage of its own proportions of the products of combustion from the furnace to the chimney. The second, to transmit through its walls the greatest allowable quantity of the heat of these products to the water; and the third purpose is, under the given conditions, to fulfil its duties better and more economically than any other device. We shall consider the last first, because it is impossible to put a flue to work until it is made, and on its material a great deal of its value will ultimately depend. The first tubes used appear to have been of copper. The tubular boiler proper was employed in France before it came into favor, even in the idea, here. The first instance of its actual use on record appears to have occurred in the case of two of Stevenson's locomotive engines, fitted in 1828 with the tubular boilers by M. Marc Seguin, engineer of the St. Etienne railway. Stephenson used copper flues in the first instance in the "Rocket." Shortly afterwards their use became habitual in America, especially for wood burning engines, and they are used in that country still, although rarely. Now copper appears at first sight to be a very excellent material for flue-tubes. It can be easily drawn into pipes; hard solder may be employed to make up the joints; its ductility is such that the tube expands under the action of the ferrules in the tube plates, and is, therefore, very easily fitted into place without any necessity for nice workmanship; and, above all, the metal is one of the best conductors of heat known, the relative thermal resistance of iron and copper being as 96 to 40. No wonder, therefore, that the use of copper flues was habitual for some time after the railway system, as applied to the conveyance of passengers, came into operation. But with the good qualities that we have named, all that is good about copper tubes begins and ends, and very little practice proved that the particles of flying coke cut them up so rapidly that they required continual replacement at a very heavy expense. It is well to bear in mind, therefore, that copper, theoretically the best, is practically the worst material ever used to make tubular flues. After the failure of copper, brass was introduced, in 1833, as a material for locomotive flues, on the Liverpool and Manchester railway; as far as we can learn, at the suggestion of Mr. Dixon, then resident engineer of that line. The copper flues lasted on an average three months; the brass two years at least. Brass possesses many of the constructive advantages of copper, while its superior hardness imparts to it the quality of permanence so desirable. Hundreds of tons of brass tubes are used, therefore yearly, for locomotive purposes. Iron tubes, though rather more difficult to set, apparently remain tight longer, and are as a rule more durable than brass; they are a great deal cheaper as well, and in consequence they are extensively used; indeed, it is questionable if they may not one day turn brass out of the market. Thus we see that either iron or brass are better calculated to answer the third great purpose of a flue than any other material. Steel may yet play an important part, of course; but, in the absence of extended experience in the use of steel flues, it would be premature to speak positively as to their value.

The second great purpose to be fulfilled by a flue is the transmission of heat to the water with which, in common parlance, it is in contact. It may be said that this is really the first object; but such is not the fact, because it is impossible that the tube should have any heat to transmit, unless the products of combustion can pass freely through it in the first instance; and the neglect of the principle involved in this statement has been the cause, time and again, of great disappointment. We can state, without hesitation, that the materials of which a flue is composed does not practically exert the smallest influence on its evaporative powers. It is true that both brass and copper are very much better conductors of caloric than iron. Yet iron flues are quite as efficient and as economical, in all that concerns the use of fuel, as either the one or the other of the dearer metals we have named. Why this should be so has excited a great deal of unnecessary and useless speculation. In point of fact, the water within a tubular boiler is never in contact with the metal of the tube. The same holds true of the heated gases. A coating of deposit of greater or lesser thickness collects after the first few days of the active life of a new boiler on the exterior of the flues, and a coating of soot forms within. The conducting powers of these two substances, really measure the calorificant value of the tube. When experiments are conducted in the laboratory on this subject, the metallic surfaces are all kept clean, perhaps bright. Could the flues of a locomotive be kept bright also, there is no doubt that the results obtained in the laboratory would be verified in practice. Thus we find that, during the first few days a brass-flued boiler always makes more steam than one the flues of which are of iron. With the appearance of a little deposit this superiority becomes evanescent. Deposit is one of the most perfect non-conductors of heat in existence, and so greatly does it obstruct the passage of caloric, that the difference between the relative values of copper and iron is as nothing, when compared with the relative values of iron, and copper, and deposit, as conductors. While each tube is enveloped in a second strongly adherent to it, and formed of lime and clay, it is a matter of very little consequence of what the tube itself is made. Even with surface condensation it is impossible, practically impossible, to keep the inside of a boiler clean. If there is not sulphate of lime, there is certain to be oxide of iron arising from the decay and corrosion of the plates. No laboratory experiments have yet been undertaken to determine the relative value of encrusted flues of different metals; simply, we presume, because it was evident beforehand that no appreciable difference would be found to exist. To put the fact in the simplest light, it is the deposit which directly imparts heat to the water with which it, and not the tube, is in contact. All that the metal of the flue can possibly do is involved in the transmission of caloric to the deposit in the first instance.

We have stated that a great number of experiments have been undertaken to determine the relative value of tube and fire-box surface. The results of these have been presented to the world as conclusive. Yet they are, one and all, untrustworthy, and to a certain extent fallacious. In 1830 Stephenson took the top off a locomotive boiler; a tube-plate separated the water in the fire-box shell from that in the barrel of the boiler. A fire was lit in the box, and the results obtained went to show that one foot of fire-box was equivalent to three of tube surface. In 1840 Mr. Dewrance performed the same experiment with a difference. He divided a small tubular boiler into seven distinct compartments, the first being fire-box, the second six inches of tube, and the remaining five, a foot each of tube, From this experiment it was deduced that the first six inches of tube were equal, square foot for square foot, to the fire-box surface; the second compartment was about one-third as effective; and for the rest, Mr. Dewrance stated that the evaporation was so small as to be practically useless. Mr. Wye Williams, not many years ago, fitted up a tube five feet long, and three inches in diameter, in his laboratory. This tube passed through a vessel divided into five compartments, each a foot long; the heat was applied by means of a ring of gas jets fitted in one end of the tube, turned down at right angles. The temperature of the waste heat was said to have been 800°, yet the boiling point was never once reached in the compartment furthest from the source of heat. Not one of these experiments possesses much in common with the conditions under which a tube works in a locomotive boiler, simply because the influence of the blast in the chimney has invariably been neglected. The caloric to be derived by conduction from heated air is very trifling, because hot air parts with caloric with great reluctance. With true flame the case is altogether different; the most intense heats known in the arts are the result of flame. The blow-pipe, the lime-light, nay, even the combustion of gas in a common paraffin lamp, are illustrations of the truth of this proposition. No one ever attempted with success to fuze platinum, or decompose the diamond, with a jet of heated air; flame has alone proved equal to the task. It is certain that water takes up heat very slowly from a gas unless in actual contact with it, and the absolute quantity of caloric which will penetrate a metal plate apparently varies nearly as the cube of the intensity of the sensible heat. Why this should be so we know not; and the entire problem has been very much overlooked by experimentalists. The relative calorific value, then, of flame is transcendent as compared with hot air, and this indisputable fact once admitted, there is no longer any difficulty in seeing wherein, Stephenson's, Dewrance's, and Wye Williams' experiments were defective. The belief obtained then, as it does now, that flame would not enter a small tube. The inventor of a safety-lamp depending on this principle for its success was not likely to dispute the question; especially with the example of the Davy-lamp—a far more perfect invention—before his eyes. We are told nothing of a forced draft in Mr. Dewrance's experiments, and from the nature of those conducted by Mr. Williams it is simply impossible that the flame could enter the tube he used. In the case of a locomotive burning coal, or even a gaseous coke, there is no doubt whatever that, with a powerful blast, flame does traverse the tubes in whole or in part of their length; the distance depending on their diameter and the force of the draft. As a datum from which deductions may be drawn, we may state that flame—true bright flame—will traverse a tube 2¾ ins. diameter, 6 feet long, at each stroke of the piston, and we feel very little doubt that in coal-burning engines the tube surface invariably possesses a higher economic value in proportion to the fire-box than when coke is burned. We said there was a fourth purpose which the tube flue might fulfil; and it is involved in permitting the combustion of gas and the conequent production of flame, to go on within it. Certainly, it is somewhat remarkable that, while Mr. Williams could not make water boil by means of air heated to 800°, steam of half the same temperature will, if conveyed through a worm, cause water to boil freely in an open vessel. It is known that locomotives with long flues of small diameter are unable to make steam without a tremendous blast. The cause has been sought in the friction of the air against the sides of the pipes. Doubtless, there is a certain amount of truth in this, but not all the truth. Flame will not enter a very small tube very far, except under strong compulsion, and there is little doubt that in such cases the value of the tube surface falls off so much in consequence of the absence of flame within them, that the evaporative powers of the boilers suffer considerably. A great number of small tubes are never so efficient individually as a smaller number of large ones. As to the relative durability, absence of priming, and efficacy, the over-flued boiler will bear no comparison with its rival.

The first great purpose of a flue is the conveyance of the products of combustion to the smoke box. The concluding sentences of the last paragraph state nearly all that can be said on this branch of the subject. The proportion which the calorimeter of a boiler—in other words, the area of its flues or tubes—should bear to the area of grate bar, is a question of considerable importance. In the case of the locomotive, it is intimately connected with the position of the blast pipe, its area, and the mode in which it is fitted. In the marine boiler the cubic contents of the furnace, the power of the draft, the quantity of the coals, &c., all exercise a considerable influence. We purpose returning to this branch of the subject at another time.

Theoretical and Practical Researches on the Formation of Positive Photographic Proofs. By MM. DAVANNE and GIRARD.

From the London Chemical News, No. 52.

Photographic images, especially those known as positive proofs, are the result of remarkable transformations, which might, unless properly studied, be considered anomalies. In each new operation the color, clearness, intensity, and solidity of the result may be very considerably varied, without the cause of the variations being apparent. These hitherto unexplained transformations may, nevertheless, be classed with ordinary chemical reactions. This is what we have endeavored to prove by a series of researches extending over six years.

To produce a positive proof the photographer takes a sheet of paper, covered with a sizing of albumen, gelatine, or starch, impregnates it with a soluble chloride, and then submits it to the sensitive action of a solution of silver. The image is then ready for insolation; placed under a negative plate it re-produces inversely the most delicate details. At this moment it is very brilliant, but will fade if not fixed by re-agents, which dissolve on the unacted salts; and its coloration would be confined to the red tints given by contact with the fixers were it not finally submitted to the action of the coloring liquids called toning agents.

These successive operations require all the attention of the chemist; we have followed them step by step, and each has revealed to us some new facts, of which we will give a succinct *résumé.*

On Paper.—It is a fact well known to photographers that proofs prepared under the same conditions, but on papers of different origin, produce tones varying extremely. The cause of these variations arise from the influence exercised by the sizing of the photographic papers. A proof on unsized paper when taken from the fixing bath is always grey and flat; while on gelatinized, albuminized, or starched paper, it always has a red brilliant tone, increasing in vigor with the abundance of the sizing. In this case a combination takes place between the sizing and the silver compounds, and this combination, a true lake color, manifests its influence towards the finishing of the proof. The fact may easily be directly demonstrated: a mixture of chloride and nitrate of silver long exposed to the light, then heated with hyposulphite of soda, leaves as residue a grey metallic powder; while the same mixture, with the addition of gelatine, albumen, or starch, gives, under the same circumstances, a matter which gradually dries under the form of a brilliant red varnish, and which is found, on analysis, to contain carbon, hydrogen, and nitrogen. This argento-organic lake plays a considerable part in obtaining a photographic image; we shall have fresh occasion to appreciate its importance when investigating the causes of the alteration of the proofs.

The Salting.—The first operation which the paper undergoes is the imbibition of a soluble chloride; for this purpose chloride of sodium is usually employed, but certain authors recommend various other metallic chlorides, attributing special qualities to them. We have shown that the differences in action of these chlorides are more apparent than real; they are always to be ascribed to the varying excess of acid with which the salts are impregnated. With any chloride very various coloring may be obtained; mixed with an excess of acid or alkali, this chloride will always give a redder tinge than if employed in a neutral state; the natural explanation of this result is to be found in the normal action of acids and alkalies on the organic matters employed for sizing.

On Rendering Sensitive.—Chlorinated and dried, the paper is then placed on a bath of nitrate of silver; three things thus result from this and the sensitive surface on coming from this bath is formed of chloride of silver, of a combination of gelatine, albumen, or starch, with nitrate of silver; and, lastly, of an excess of free nitrate of silver. The presence of these three substances is indispensable to the production of a good proof; chloride of silver alone gives but a dull and superficial image, but it produces the image rapidly; nitrate in excess gives the necessary depth of this image, and the argentico-organic lake gives it the characteristic red color. The sensitivizing bath may vary in richness, and we have carefully studied the effect of these variations; it may be neutral acid or alkaline; in the two latter cases the effect is the same as if the acid or alkali had been added to the chloride bath.

On Insolation.—To determine the effect of light on the sensitive surface, the composition of which we have given, is certainly, from a theoretical point of view, the most important part of our researches. It is universally admitted that chloride of silver exposed to the solar rays undergoes decomposition and disengages part of its chlorine; but the question is really much more complex than at first appears, and it is necessary to ascertain not only what becomes of the chloride, but also what becomes of the argentico-organic combination of free nitre.

First as to the chloride. It has long been thought that light reduced this body to the state of sub-chloride Ag_2Cl. We have shown that such is not the case, and we stated that the decomposed chloride separates entirely as chlorine and metallic silver. We have established this, the principal point, by first proving that the product of the action of light on chloride of silver is soluable in hot nitric acid, while the essential property of sub-chloride Ag_2Cl is its insolubility in this re-agent, and that this product, freed by hyposulphite of soda from the unreduced chloride of silver, contains no trace of chlorine.

It has certainly been objected to this last proof that hyposulphite of soda used as a fixing agent might have decomposed the sub-chloride Ag_2Cl into the chloride $AgCl$, which would there dissolve into metallic silver; but the only good experiment cited in support of this hypothesis is the change in the color of the insolated proof when placed in contact with the fixing agent. Now, we have ascertained that this change of color is due to quite another cause, to a hydratation of the argentico-organic lake, and that this result is to be obtained by simply exposing the proof to the vapors of boiling water. Chloride of silver, then, is transformed by the action of light into chlorine and metallic silver.

It is the disengagement of chlorine from this decomposition which makes nitrate of silver play so important a part in positive photography. Thus, as we have already observed, a proof obtained with chloride of silver only is always tame and effective; in presence of excess of nitrate, on the contrary, it acquires great brilliancy. This result is easily explained; in fact, the action of light on a uniform surface of chloride of silver is soon limited by the opaque layer produced by the superficial reduction of the argentic compound; but if this compound is mixed with free nitrate of silver then, beside the portions reduced, and under the influence of the chlorine, they disengage, there are formed new quantities of chloride, which may then be attacked by light, because before this, in the state of nitrate, these portions occupied a particular place, and the reduced silver did not hitherto recover them; so that instead of a dull image several excessive layers are formed, which give to the design the required depth.

At the same time that the chloride of silver is reduced to a metallic state the argentico-organic combination is also reduced, forming a sort of insoluble lake, which, hydrating by contact with alkaline fixers, communicates a very pronounced red coloring to the proof.

The applications of these fixers is the second phase in the manipulations required in positive photography.—*Comptes-Rendus*, lviii., 634.

Description of a New Barometer.

From the Journal of the Society of Arts, No. 593.

This barometer consists of a column of mercury placed in a glass tube hermetically sealed at the top, and perfectly open at the bottom. The lower half of the tube is of larger bore than the upper. If a column of mercury, of exactly the length which the atmosphere, at the given time, is capable of supporting, were placed in a tube of glass, hermetically sealed at the top, of equal bore from end to end, the mercury would be held in suspense; but immediately the pressure of the atmosphere increased, the mercury would rise towards the top of the tube, and remain there till, on the pressure decreasing, it would fall towards the bottom, and the portion which the atmosphere was unable to support would drop out. If, however, the lower half of the tube be made a little larger in the bore than the upper, then, when the column falls, the upper portion passes out of the smaller part of the tube into the larger, and, owing to the greater capacity of the latter, the lower end of the column of mercury does not sink to the same extent as the upper end, and the column, as a matter of course, becomes shorter. This falling will continue until the column is reduced to that length which the atmosphere is capable of supporting, and the scale attached thus registers this fall, or what is ordinarily termed the height of the barometer. From the above description it will be evident that, by merely varying the proportions in the sizes of the two parts of the tube a scale of any length can be obtained. For example, if the tubes are very nearly the same size in bore, the column has to pass through a great distance before the necessary compensation takes place, and a very long scale may thus be obtained, say ten inches for every one inch rise and fall in the ordinary barometer. But if the lower tube is made much longer than the upper, the mercury passing into it quickly compensates, and a small scale, say from two to three inches for every inch, is obtained. To ascertain how many inches this rise and fall for an ordinary inch of the barometer would be, the inventor, Mr. Hicks, of Hatton-garden, attaches his barometer with a standard barometer to an air-pump receiver, and thus ascertains the scale for every inch, from 31 to 27 inches. On the same principle, as regards the tube, the inventor has constructed an absolute standard barometer graduating the scale from the centre, and reading it off at each end of the column with two verniers, to the one-thousandth of an inch. To ascertain the height of the barometer graduated in this way, he takes a reading of the upper surface of the column of mercury with one vernier, then of the lower surface in the same way with the other vernier, and by adding the two readings together, he gets the exact length of the column of mercury supported in the air, which is the true height of the barometer at the time. Gay-Lussac's pipette is introduced into the centre of the tube to prevent the possibility of any air passing up into the top. Such a barometer is convenient for measuring mountain heights, being very portable and getting rid of the cistern, and the corrections necessary when this is used.

On the Molecular Mobility of Gases. By THOMAS GRAHAM, F.R.S.

The molecular mobility of gases is here considered in reference chiefly to the passage of gases, under pressure, through a thin porous plate or septum, and to the partial separation of mixed gases which can be effected, as will be shown, by such means. The investigation arose out of a renewed and somewhat protracted inquiry regarding the diffusion of gases (depending upon the same molecular mobility), and has afforded certain new results which may prove to be of interest in a theoretical as well as in a practical point of view.

In the diffusiometer, as first constructed, a plain cylinderical glass tube, rather less than an inch in diameter and about ten inches in length, was simply closed at one end by a porous plate of plaster of paris, about one-third of an inch in thickness, and thus converted into a gas receiver.* A superior material for the porous plate is now found in the artificially compressed graphite of Mr. Brockedon, of the quality used for making writing-pencils. This material is sold in London in small cubic masses about 2 inches square. A cube may easily be cut into slices of a millimetre or two in thickness by means of a saw of steel spring. By rubbing the surface of the slice without wetting it upon a flat sand-stone, the thickness may be further reduced to about one-half of a millimetre. A circular disk of this graphite, which is like a wafer in thickness but possesses considerable tenacity, is attached by resinous cement to one end of the glass tube above described, so as to close it and form a diffusiometer. The tube is filled with hydrogen gas over a mercurial trough, the porosity of the graphite plate being counteracted for the time by covering it tightly with a thin sheet of gutta percha. On afterwards removing the latter, gaseous diffusion immediately takes place through the pores of the graphite. The whole hydrogen will leave the tube in forty minutes or an hour, and is replaced by a much smaller proportion of atmospheric air (about one-fourth), as is to be expected from the law of the diffusion of gases. During the process, the mercury will rise in the tube, if allowed, forming a column of several inches in height—a fact which illustrates strikingly the intensity of the force with which the interpenetration of different gases is effected. The native or mineral graphite is of a lamelar structure, and appears to have little or no porosity. It cannot be substituted for the artificial graphite as a diffusion-septum. Unglazed earthenware comes next in value to graphite for this purpose.

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* "On the Law of the Diffusion of Gases," Transactions of the Royal Society of Edinburgh, vol. xii. p. 222; or Philosophical Magazine, 1834, vol. ii. pp. 175, 269, 351.

The pores of artificial graphite appear to be really so minute, that a gas *in mass* cannot penetrate the plate at all. It seems to be molecules only which can pass; and these may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestate movement of molecules which is now generally recognised as an essential property of the gaseous condition of matter.

According to the physical hypothesis now generally received,* a gas is represented as consisting of solid and perfectly elastic spherical particles or atoms, which move in all directions, and are animated with different degrees of velocity in different gases. Confined in a vessel, the moving particles are constantly impinging against its sides and occasionally against each other, and such collisions take place without any loss of motion, owing to the perfect elasticity of the particles. Now if the containing vessel be porous, like a diffusiometer, then gas is projected through the open channels, by the atomic motion described, and escapes. Simultaneously the external air or gas, whatever it may be, is carried inwards in the same manner, and takes the place of the gas which leaves the vessel. To the same atomic or molecular movement is due the elastic force, with the power to resist compression, possessed by gases. The molecular movement is accelerated by heat and retarded by cold, the tension of the gas being increased in the first instance and diminished in the second. Even when the same gas is presented both within and without the vessel, and is therefore in contact with both sides of the porous plate, the movement is sustained without abatement—molecules continuing to enter and leave in equal number, although nothing of the kind is indicated by change of volume or otherwise. If the gas in communication be different but possess sensibly the same specific gravity and molecular velocity, as nitrogen and carbonic oxide do, an interchange of molecules also takes place without any change in volume. With gases opposed of unequal density and molecular velocity, the amount of penetration ceases of course to be equal in both directions.

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* D. Bernoulli, J. Herapath, Joule, Kronig, Clausius, Clerk Maxwell, and Cazin. The merit of reviving this hypothesis and first applying it to the facts of gaseous diffusion, is fairly due to Mr. Herapath. See "Mathematical Physics," in two volumes, by John Herapath, Esq., (1847.)

These observations are preliminary to the consideration of the passage through a graphite plate, in one direction only, of gas under pressure, or under the influence of its own elastic force. It is to be supposed that a vacuum is maintained on one side of the porous septum, and that air or some other gas, under a constant pressure, is in contact with the other side. Now a gas may pass into a vacuum, in three different modes, or in two modes besides that immediately before us.

1. The gas may enter the vacuum by passing through a minute aperture in a thin plate, such as a puncture in platinum foil made by a fine steel point. The rate of passage of different gases is then regulated by their specific gravities, according to a pneumatic law which was deduced by Professor John Robison from Torricelli's well-known theorem of the velocity of efflux of fluids. A gas rushes into a vacuum with the velocity which a heavy body would acquire by falling from the height of an atmosphere composed of the gas in question, and supposed to be of uniform density throughout. The height of the uniform atmosphere will be inversely as the specific gravity of the gas, the atmosphere of hydrogen, for instance, sixteen times higher than that of oxygen. But as the velocity acquired by a heavy body in falling is not directly as the height, but as the square root of the height, the rate of flow of different gases into a vacuum will be inversely as the square root of their respective densities. The velocity of oxygen being 1, that of hydrogen will be 4, the square root of 16. This law has been experimentally verified.* The times of the effusion of gases, as I have spoken of it, are similar to those of the law of molecular diffusion; but it is important to observe that the phenomena of effusion and diffusion are distinct and essentially different in their nature. The effusion movement affects masses of gas, the diffusion movement affects molecules; and a gas is usually carried by the former kind of impulse with a velocity many thousand times greater than by the latter. The effusion velocity of air is the same as the velocity of sound.

2. If the aperture of efflux be in a plate of increased thickness, and so becomes a tube, the effusion-rates of gases are disturbed. The rates of flow of different gases, however, assume again a constant ratio to each other when the capillary tube is considerably elongated, when the length exceeds the diameter at least 4000 times. These new proportions of efflux are the rates of the "Capillary Transpiration of Gases."† The rates were found to be the same in a capillary tube composed of copper as they are in a tube of glass, and appear to be independent of the material of the capillary. A film of gas no doubt adheres to the inner surface of the tube, and the friction is really that of gas upon gas, and is consequently unaffected by the nature of the tube-substance. The rates of transpiration are not governed by specific gravity, and are indeed singularly unlike the rates of effusion.

The transpiration-velocity of oxygen being 1, that of chlorine

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* "On the Motion of Gases," Phil. Trans. 1846 p. 573.

† Phil. Trans. 1846, p. 591, and 1849, p. 349.

1·5, that of hydrogen 2·26, of ether vapor at low temperatures the same or nearly the same number as hydrogen, of nitrogen and carbonic oxide half the velocity of hydrogen, of olefiant gas, ammonia, and cyanogen 2 (double or nearly double that of oxygen), of carbonic acid 1·376, and of the gas of marshes 1·815. In the same gas the transpirability of equal volumes increases with density, whether occasioned by cold or pressure. The transpiration-ratios of gases appear to be in constant relation with no other known property of the same gases, and they form a class of phenomena remarkably isolated from all else at present known of gases.

There is one property of transpiration immediately bearing upon the penetration of the graphite plate by gases. The capillary offers to the passage of gas a resistance analogous to that of friction, proportional to the surface, and consequently increasing as the tube or tubes are multiplied in number and diminished in diameter, with the area of discharge preserved constant. The resistance to the passage of a liquid through a capillary was observed by Poiseuille to be nearly as the fourth power of the diameter of the tube. In gases the resistance also rapidly increases; but in what ratio, has not been observed. The consequence, however, is certain, that as the diameter of the capillaries may be diminished beyond any assignable limit, so the flow may be retarded indefinitely, and caused at last to become too small to be sensible. We may therefore have a mass of capillaries of which the passages form a large aggregate, but which are individually too small to permit a sensible flow of gas under pressure. A porous solid mass may possess the same reduced penetrability as the congeries of capillary tubes. Indeed the state of porosity described appears to be more or less closely approached by all loosely aggregated mineral masses, such as lime-plaster, stucco, chalk, baked clay, non-crystalline earthy powders like hydrate of lime or magnesia compacted by pressure, and in the highest degree perhaps by artificial graphite.

3. A plate of artificial graphite, although it appears to be practically impenetrable to gas by either of the two modes of passage previously described, is readily penetrated by the agency of the molecular or diffusive movement of gases. This appears on comparing the time required for the passage of equal volumes of different gases under a constant pressure. Of the following three gases, oxygen, hydrogen, and carbonic acid, the time required for the passage of an equal volume of each through a capillary glass tube, in similar circumstances as to pressure and temperature, was formerly observed to be as follows:—

	Time of capillary transpiration.
Oxygen,	1
Carbonic acid,	0·72
Hydrogen,	0·44

Through a plate of graphite, of half a millimetre in thickness, the same gases were now observed to pass, under a constant pressure of a column of mercury of 100 millimetres in height, in times which are as follows:—

	Time of molecular passage.	Square root of density (oxygen 1).
Oxygen,	1	1
Hydrogen,	0·2472	0·2502
Carbonic acid,	1·1886	1·1760

It appears then that the times of passage through the graphite plate have no relation to the capillary transpiration-times of the same gases first quoted above. The new times in question, however, show a close relation to the square roots of the densities of the respective gases, as is seen in the last Table; and so far they agree with theoretical *times of diffusion* usually ascribed to the same gases.

The experiments were varied by causing the gases to pass into a Torricellian vacuum, and consequently under the full pressure of the atmosphere. The times of penetration of equal volumes of gases were now:—

	Times.	$\sqrt{\text{Density}}$.
Oxygen,	1	1
Air,	0·9501	0·9507
Carbonic acid,	1·1860	1·1760
Hydrogen,	0·2505	0·2502

This penetration of the graphite plate by gases appears to be entirely due to their own proper molecular motion, quite unaided by transpiration. It seems to offer the simplest possible exhibition of the molecular or diffusive movement. This pure result is to be ascribed to the wonderfully fine porosity of the graphite. The interstitial spaces, or channels, appear to be sufficiently small to extinguish transpiration, or the passage of masses, entirely. The graphite becomes a molecular sieve, allowing molecules only to pass through.

With a plate of stucco, the penetration of gases under pressure is very rapid, and the volumes of air and hydrogen passing in equal times are as 1 to 2·891, which is a number for hydrogen intermediate between its transpiration-volume 2·04 and diffusion-volume 3·8, showing that the passage through stucco is a mixed result.

With a plate of biscuitware, 2·2 millimetres in thickness, the volume of hydrogen rose to 3·754 (air=1), approaching closely to 3·8, the molecular ratio.

The rate of passage of a gas through graphite appeared also to be closely proportional to the pressure.

Further, hydrogen was found to penetrate through a graphite plate into a vacuum, with sensibly the same absolute velocity as it diffused into air, establishing the important fact that the impelling force is the same in both movements. The molecular mobility may therefore be spoken of as the diffusive movement of gases; the passage of gas through a porous plate into a vacuum, as diffusion in one direction or single diffusion; and ordinary diffusion, or the passage of two gases in opposite directions, as double, compound, or reciprocal diffusion.

Atmolysis.—A partial separation of mixed gases and vapors of unequal diffusibility can be effected by allowing the mixture to permeate through a graphite plate into a vacuum, as was to be expected from the preceding views. As this method of analysis has a practical character and admits of wide application, it may be convenient to distinguish it by a peculiar name. The amount of the separation is in proportion to the pressure, and attains its maximum when the gases pass into a nearly perfect vacuum. A variety of experiments were made on this subject, of which perhaps the most interesting were those upon the concentration of the oxygen in atmospheric air. When a portion of air confined in a jar is allowed to penetrate into a vacuum through graphite or unglazed earthenware, the nitrogen should pass more rapidly than the oxygen in the proportion of 1·0668 to 1 and the proportion of oxygen be proportionally increased in the air left behind in the jar. The increase in the oxygen actually observed when the air in the jar was reduced from 1 volume

To 0·5	volume,	was 0·48	per cent.
0·25	"	" 0·98	"
0·125	"	" 1·54	"
0·0625	"	" 2·02	"

Or, the oxygen increased from 21 to 23·02 per cent. in the last sixteenth part of air left behind in the jar.

The most remarkable effects of separation are produced by means of the *tube atmolyser*. This is simply a narrow tube of unglazed earthenware, such as a tobacco-pipe stem two feet in length, which is placed within a shorter tube of glass and secured in its position by corks, so as to appear like a Liebig's condenser. The glass tube is placed in communication with an air-pump, and the annular space between the two tubes is maintained as nearly vacuous as possible. Air or any other mixed gas is then allowed to flow in a stream along the clay tube, and collected as it issues. The gas so atmolysed is of course reduced in volume, much gas penetrating through the pores of the clay tube into the air-pump vacuum; and the slower the gas is collected the greater the proportional loss. In the gas collected, the denser constituent of the mixture is thus concentrated in an arithmetical ratio, while the volume of the gas is reduced in a geometrical ratio. In one experiment the proportion of oxygen in the air after traversing the atmolyser was increased to 24·5 per cent., or 16·7 upon 100 oxygen originally present in the air. With gases differing so much in density and diffusibility as oxygen and hydrogen, the separation is of course much more considerable. The explosive mixture of two volumes of hydrogen and one volume of oxygen, gave oxygen containing only 9·3 per cent. of hydrogen, in which a taper burned without explosion; and with equal volumes of oxygen and hydrogen, the proportion of the latter was easily reduced from 50 to 5 per cent.

Interdiffusion of Gases—double diffusion.—The diffusiometer was much improved in construction by Prof. Bunsen, from the application of a lever arrangement to raise and depress the tube in the mercurial trough. But the mass of stucco forming the porous plate in his instrument was too voluminous, in my opinion, and, from being dried by heat, had probably detached itself from the walls of the glass tube. The result obtained of 3·4 for hydrogen, air being 1, is, I understand, no longer insisted upon by that illustrious physicist. It is indeed curious that my old experiments generally rather exceeded than fell short of the theoretical number for hydrogen $\sqrt{\frac{1}{0\cdot06926}}=3\cdot7997$. With stucco as the material, the cavities in the porous plate form about one-fourth of its bulk, and affect sensibly the ratio in question, according as they are or are not included in the capacity of the instrument. Beginning the diffusion always with these cavities filled with hydrogen, the numbers now obtained with a stucco plate of 12 millimetres in thickness, dried without heat, were 3·783, 3·8, and 3·739 when the volume of the cavities of stucco is added to the air and hydrogen, and 3·931, 3·949, and 3·883 when such addition is not made to these volumes. The graphite plate, on the other hand, being thin, and the volume of its pores too minute to require to be taken into account, its action is not attended with the same uncertainty. With a graphite plate of 2 millimetres in thickness, the number for hydrogen into air was 3·876, and of hydrogen into oxygen 4·124, instead of 4. With a graphite plate of 1 millimetre in thickness, hydrogen gave 3·993 to air 1. With a graphite plate of 0·5 millimetre in thickness, the proportional number for hydrogen to air rose to 3·984, 4·067. A similar departure from the theoretical number was observed when hydrogen was diffused into oxygen or carbonic acid, instead of air. All these experiments were made over mercury and with dried gases. It appears that the numbers are most in accordance with theory when the graphite plate is thick, and the diffusion slow in consequence. If the diffusion be very rapid, as it is with the thin plates, something like a current is possibly formed in the channels of the graphite, taking the direction of the hydrogen and carrying back in mass a little air, or the slower gas, whatever it may be. I cannot account otherwise for the slight predominance which the lighter and faster gas appears to acquire in diffusing through the porous septum.

Speculative ideas respecting the constitution of matter.—It is conceivable that the various kinds of matter, now racognised as different elementry substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies. We know the anxiety with which this point was investigated by Newton, and the care he took to ascertain that every kind of substance, "metals, stones, woods, grain, salts, animal substances, &c.," are similarly accelerated in falling, and are therefore equally heavy.

In the condition of gas, matter is deprived of numerous and varying properties with which it appears invested when in the form of a liquid or solid. The gas exhibits only a few grand and simple features. These again may all be dependent upon atomic and molecular mobility. Let us imagine one kind of substance only to exist, ponderable matter; and further, that matter is devisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements as they have been considered.

What has already been said is not meant to apply to the gaseous volumes which we have occasion to measure and practically deal with, but to a lower order of molecules or atoms. The combining atoms hitherto spoken of are therefore not the molecules of which the movement is sensibly affected by heat, with gaseous expansion as the result. The gaseous molecule must itself be viewed as composed of a group or system of the preceding inferior atoms, following as a unit laws similar to those which regulate its constituent atoms. We have indeed carried one step backward and applied to the lower order of atoms ideas suggested by the gaseous molecule, as views derived from the solar system are extended to the subordinate system of a planet and its satellites. The advance of science may further require an indefinite repetition of such steps of molecular division. The gaseous molecule is then a re-production of the inferior atom on a higher scale. The molecule or system is reached which is affected by heat, the diffusive molecule, of which the movement is the subject of observation and measurement. The diffusive molecules are also to be supposed uniform in weight, but to vary in velocity of movement, in correspondence with their constituent atoms. Accordingly the molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

But further, these more and less mobile or light and heavy forms of matter have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic groupe, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. This is chemical combination. It is directly an affair of volume, and only indirectly connected with weight. Combining weights are different, because the densities, atomic and molecular, are different. The volume of combination is uniform, but the fluids measured vary in density. This fixed combining measure—the *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other "elements."

To the preceding statements respecting atomic and molecular mobility, it remains to be added that the hypothesis admits of another expression. As in the theory of light we have the alternative hypothesis of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undulate. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens that portion of matter with an individual existence, and constitutes it a distinct substance or element.

With respect to the different states of gas, liquid, and solid, it may be observed that there is no real incompatibility with each other in these physical conditions. They are often found together in the same substance. The liquid and the solid conditions supervene upon the gaseous condition rather than supersede it. Gay-Lussac made the remarkable observation that the vapors emitted by ice and water, both at 0° C., are of exactly equal tension. The passage from the liquid to the solid state is not made apparent in the volatility of water. The liquid and solid conditions do not appear as the extinction or suppression of the gaseous condition, but something *superadded* to that condition. The three conditions (or constitutions) probably always coexist in every liquid or solid substance, but one predominates over the others. In the general properties of matter we have, indeed, to include still further (1) the remarkable loss of elasticity in vapors under great pressure, which is distinguished by Mr. Faraday as the Caignard-Latour state, after the name of its discoverer, and is now undergoing an investigation by Dr. Andrews, which may be expected to throw much light upon its nature; (2) the colloidal condition or constitution, which intervenes between the liquid and crystalline states, extending into both and affecting probably all kinds of solid and liquid matter in a greater or less degree. The predominance of a certain physical state in a substance appears to be a distinction of a kind with those distinctions recognised in natural history as being produced by unequal development. Liquefaction or solidification may therefore not involve the suppression of either the atomic or the molecular movement, but only the restriction of its range. The hypothesis of atomic movement has been elsewhere assumed, irrespective of the gaseous condition, and is applied by Dr. Williamson to the elucidation of a remarkable class of chemical re-actions which have their seat in a mixed liquid.

Lastly, molecular or diffusive mobility has an obvious bearing upon the communication of heat to gases by contact with liquid or solid surfaces. The impact of the gaseous molecule, upon a surface possessing a different temperature, appears to be the condition for the transference of heat, or the heat movement, from one to the other. The more rapid the molecular movement of the gas the more frequent the contact, with consequent communication of heat. Hence, probably, the great cooling power of hydrogen gas as compared with air or oxygen. The gases named have the same specific heat for equal volumes; but a hot object placed in hydrogen is really *touched* 3·8 times more frequently than it would be if placed in air, and 4 times more frequently than it would be if placed in an atmosphere of oxygen gas. Dalton had already ascribed this peculiarity of hydrogen to the high "mobility" of that gas. The same molecular property of hydrogen recommends the application of that gas in the air engine, where the object is to alternately heat and cool a confined volume of gas with rapidity.

Proceedings of the Royal Society, No. 56.

Artificial Sunlight.

From the London Mechanics' Magazine, April, 1864.

We know that for many year past attempts have been made to evolve from natural elements a gas or flame which, on a small scale, should equal in quality and intensity the light emitted from the sun. Abroad, as well as at home, this has been a haunting thought in the brain of chemists. Innumerable experiments, with more or less success for their results, have been made from time to time, and there is no doubt that eventually perseverance will meet its almost unfailing reward—achievement of the object sought.

So long back as 1859, Professor Bunsen and Professor Roscoe suggested that the fusion of the metal magnesium might prove to be the solution of the philosophical problem, and now Mr. Sondstadt is actually commencing to manufacture that metal on a large scale for photographic purposes. It has been demonstrated that, by burning magnesium wire in a spirit or oil lamp, an illuminating power of great brilliancy might be gained. The two professors named, long since examined the photo-chemical action of the sun, compared with a terrestrial source of light, and this latter was that effected by the combustion of magnesium wire. The application of this light may become, it is easy to perceive, of vast importance beyond its photographic uses. A burning magnesium wire, of the thickness of 0·297 millimetre, evolves as much light as seventy-four stearine candles, of which five go to the pound. In order to produce a light equal to that of seventy-four such candles burning for ten hours, and in which 20 lbs. of stearine would be consumed, 72·2 grammes of magnesium wire would be required. The magnesium wire is prepared by forcing out the metal from a heated steel press, having a fine opening at the bottom. For the purpose of consumption, it may be rolled up in coils on a spindle, which, by the agency of clock-work, or weights above, could be made to revolve. A pair of feeding rolls would push the end of the wire forward, at a rate commensurate with the speed of its combustion.

Magnesium is not in itself costly at present, but there is no doubt that the efforts of Mr. Sondstadt and others who are devoting attention to the subject will lessen the expense of its production by improved manipulation.

So far as the usefulness of the discovery is concerned in relation to photography, we have the following testimony from Mr. Brothers, of Manchester:—"The result of an experiment I have just tried, is, that in fifty seconds, with the magnesium light, I have obtained a good negative copy of an engraving—the copy being made in a darkened room. Another copy was made in the usual way, in daylight, and in fifty seconds the result was about equal to the negative taken by the artificial light." Who shall say, therefore, that at some not distant day, nature and science may not place at our disposal a substitute for the bright orb of day—an artificial sun.

Boiler Explosions, and their Prevention.

From the London Mining Journal, No. 1498.

An important letter upon this subject has been addressed to the Editor of the *Times* by Mr. Joseph Hopkinson, of the Britannia Works, Huddersfield, in which he points out the disadvantage of official inspection, whether conducted by the government or by commercial associations. He maintains that many boiler explosions have taken place from the interference of the inspector. He admits that inspection may be the means of lessening these catastrophes, but considers that the class of inspection required must greatly differ from the system carried out by the Manchester Association, which he avers is not only retrograde in its tendency, but is a positive evil. It throws the responsibility into the lap of an irresponsible body, who have merely made boiler explosions a game of speculation. His experience with these societies proves the remarks made by Mr. Jewison, coroner for the Leeds district, that it is "A system by which boiler owners are lulled into fancied security." He continues, that he would ask these societies how many explosions have taken place of boilers under their charge, and how many persons injured, which would have been prevented had there been a proper valve? how many fusible plugs they have found inoperative? and why that apparatus is recommended by one of the Manchester associations, and condemned by the other? and after the report made by the Franklin Institute, declaring its inefficiency, from experiments made. He would ask them why they recommend twice as many fittings to a boiler as is necessary? why they would have so many holes pierced in a boiler for the adoption of the various "philosophical toys" they recommend? He observes that the operative engineers of Oldham and neighborhood adopt a very different course to that pursued by the Manchester Association, which is by the formation of an institution, where lectures and information on matters relating to the steam engine and boiler are imparted to them, and they choose a large body of men of sufficient experience from their own society, who act as inspectors for their neighbors' boilers. The result has been, by this spirit of self-government and the adoption of proper fittings, that not one explosion has taken place—a very different result to that of these great companies, seemingly got up under more favorable auspices. The officialism of red tape and indifference, as well as the payment of useless salaries, is avoided, whilst the results are far more conducive to the interests of society. Some parties, however, adopt a different course to that pursued either by the Manchester Association or the engineers of Oldham, and which they say is preferable to any yet devised. The method is, by agreeing with a practical boiler maker to examine the boiler thoroughly, whenever called upon, at a fixed sum per boiler examination; and should he succeed in discovering any faulty place he is entrusted with the repairs. Mr. Hopkinson's conclusions are, that the only safeguard against boiler explosions is to be found in efficient self-acting mechanical contrivances, and declares that boiler explosions can be prevented from all causes except that of a boiler where it is too weak to withstand its ordinary working pressure; and he is prepared to attach to any boiler in London, or elsewhere, apparatus such as are in use upon the boilers at the Royal Arsenal, Woolwich, and upon the boilers of Messrs. John Brown & Co., armor-plate manufacturers, Sheffield, 42 in number, and 33 at Messrs. Crossley & Co., Halifax, and the whole of Messrs. Naylor, Vickers & Co.'s boilers, Sheffield; in fact, to 4000 boilers in this and other countries; and he offers £200 to any person who can demonstrate the possibility of exploding a boiler from over-pressure of steam, or deficiency of water, if the boiler be fitted with this patent compound safety-valve, the warnings of which, if disregarded, put a stop to the working of the engine, and render boiler explosions impossible; and what is still of great importance, its action can be stopped only by again supplying the water to the boiler.

A New Process of Engraving.

From the London Mechanics' Magazine, May, 1864.

A M. Dulos has recently invented a new process of engraving, which is described by the *Moniteur Scientifique*, as follows:—A copper plate, on which the design has been traced with lithographic ink, receives, by the action of the pile, a deposit of iron on the parts untouched by the ink; the ink having been removed by means of benzine, the white portions of the design are represented by the layer of iron, and the black by the copper itself; the plate is then plunged into a bath of cyanide of silver, under a galvanic current, and the silver is deposited on the copper only. In this condition mercury is poured over the plate, which attaches itself to the silvered portions only, appearing in relief, and taking the place of the lithographic ink. Then take, in plaster or melted wax, an imprint, the cast of which, presenting the counterpart of the projections of mercury, gives a kind of copperplate engraving. This cast has not sufficient strength to bear the press; but by metallizing the mould, and depositing upon it, electro-chemically, a layer of copper, we obtain an exact re-production of the original projections of mercury, and, in some sort, of a matrix by means of which impressions of the plate may be produced *ad infinitum.*

For typographic engraving (figures in relief), the plate of copper should receive, on leaving the hands of the designer, a layer of silver, deposited only on the parts untouched by the lithographic ink; the ink is removed by benzine, the surfaces first covered by the design are oxidized, and the treatment above described is continued. At the end of the operation the raised portions of the electro-chemical plate intended for the impression will be found to correspond with the tracing of the design, and the hollow portions with the thickenings raised about the design by the mercury.

This process, which is the starting-point and the basis of M. Dulos' invention, has led him to the discovery of some more simple methods, which have led to important practical results, the fusible metal or amalgam of copper substituted for mercury giving rapid and remarkably perfect results.

Artificial Light and Lighting Materials. By B. H. Paul, Esq.

From the Journal of the Society of Arts, No. [illegible].

(Continued from page 152.)

Illuminating Power.—In the case of all kinds of lighting materials, the light produced when they are burnt, is due to their containing, or yielding when heated, olefiant gas, or other hydrocarbon gases, which are equivalent to it as regards the production of light. In the production of artificial light by any of the means that have been hitherto commonly used, it is always the result of the intense ignition of solid particles of carbon. Luminiferous hydro-carbon gases and vapors, which, for convenience sake, may be collectively represented by the term olefiant gas, all agree in the character of being decomposed at a temperature higher than that requisite for their production, in such a manner that the greater part of the carbon they contain, is set free in a state of very minute division. This is an essential character of these luminiferous substances. In any ordinary luminous flame the particles of carbon so separated become intensely heated, and emit light while being burnt, giving to the flame its whiteness and brilliancy.

The production of heat is, therefore, an essential preliminary to the production of light, and it is also equally necessary for the production of light that the heat produced should act upon some solid substance. The separation of minutely-divided carbon from the luminiferous hydro-carbons, when they are heated, provides this essential condition for the production of light. Gases or vapors which do not yield any solid substance when burnt, do not afford a luminous flame, or at most, only a feeble light. Thus, for instance, hydrogen and marsh gas, or sulphur, burn without evolving any considerable light. Olefiant gas, on the contrary, burns with an intensely white luminous flame. At the same time it is indispensable that the solid particles separated in an ordinary flame should not retain their solid state, but should, by combustion, be converted into gas. The carbon separated in the flame of ordinary lighting materials, burns and is converted into carbonic acid gas. If this were not the case every flame would constantly produce a shower of dust.

The greater the number of solid heated particles in a flame, the greater will be the light it evolves, consequently, the amount of light capable of being produced by various hydro-carbon gases and vapors, or their illuminating power, is directly proportionate to the amount of carbon contained in a given volume of the gas or vapor, and capable of being separated during combustion. Taking olefiant gas as the standard of comparison, and referring to the table of the several hydro-carbon oils constituting petroleum and coal oil, it will be seen that the individual members of the series differ very considerably in this particular, and the figures expressing the relative amount of carbon also express the relative illuminating power of equal volumes of their vapors.

It is probable that in burning the vapor of any one of these substances the general nature of the decomposition which takes place consists in the production of marsh gas which burns, and the separation of all the carbon over and above that requisite for the formation of marsh gas. Consequently, in the flame of the substance, No. 4 of the table, there would be nearly twice as much carbon separated as in the flame of olefiant gas. Separation of carbon to such an extent would be more than sufficient for the production of light under ordinary circumstances, and would be accompanied by a tendency to the production of smoke, in consequence of some of the carbon escaping combustion. This tendency would of course be greater in proportion to the increased amount of carbon in the vapor, and although the illuminating capability would be greater in the same proportion, it would not be possible to realize that capability except under special conditions.

There is, therefore, a practical limit, in regard to the mode in which lighting materials are generally used, beyond which an increase in the amount of carbon in the material would be rather prejudical than otherwise.

In the case of gas, it is never anything like an approximation to pure olefiant gas that is used, but only a mixture of it with other gases, containing from 5 to upwards of 20 per cent. of olefiant gas. The following gives the composition of coal-gas in various localities:—

	Manchester. Roscoe.	Greenock. T. Thomson.			Newcastle. T.Richardson		Boghead. Fyfe.
Olefiant gas, .	8·84	14·50	17·50	20·00	10·19	9·25	27
Marsh gas, .	34·90	66·49	59·94	47·77	31·35	36·05	. .
Hydrogen, .	45·58	12·29	11·46	17·32	28·80	30·17	. .
Carbonic oxide,	6·64	7·07	12·	11·76	16·28	11·42	. .
Carbonic acid, .	3·67	. .	. .	. .	. .	. .	. .
Nitrogen, . .	2·46	. .	. .	. .	. .	. .	. .
Sulph. hydrogen,	0·29	. .	. .	. .	. .	. .	. .

Of these gases, the first three after olefiant gas, are the only ones that take any part in the production of light, the others being mere impurities. Those gases, however, do not contribute directly to the production of light to any extent, but being themselves combustible, serve in part, to produce heat requisite for decomposing the olefiant gas, and rendering the particles of carbon separated from it luminous. They also serve to prevent the flame from smoking, by diluting the olefiant gas, and thus admitting of its being brought more intimately in contact with the air required for perfect combustion. They also give bulk to the flame, and thus diminish the concentration of the light which would take place if the olefiant gas were burnt without any admixture.

In all these respects each one of the three gases answers the purpose required of it equally well or nearly so. There are, however, other particulars in regard to which there is a very considerable difference in their fitness for these purposes. This difference consists in the respective capability of these gases for producing heat and carbonic acid by their combustion. Any amount of heat produced by burning any lighting material, over and above that requisite for giving the necessary temperature to the flame, is obviously objectionable, since it would have the effect of heating the atmosphere of the space lighted and rendering it oppressive. Since all lighting materials contain carbon, and by their combustion abstract oxygen from the air, the production of carbonic acid is, to some extent, a necessary result of the use of any such material; but, on account of the prejudicial influence of this gas, it is highly desirable that the quantity produced, for a given quantity of light, should be as small as possible. In both these respects there is a very wide difference between the effects of the three gases which are generally contained in coal gas as the dilutants of the olefiant gas. This will be seen from the following table:—

	Heating power for equal volumes.	Relative volume of Oxygen consumed.		Volume of Carbonic acid produced.	Temperature of Flame.
			Air.		Fahr.
Hydrogen, . .	4,299	0·5	2·5	0	4966,°
Carbonic acid, . .	3,234	0·5	2·5	1	5619
Marsh gas, . .	14,634	2·0	10·0	1	4940
Olefiant gas, . .	. .	3·0	15 0	2	. .

This comparison serves to show that although hydrogen produces more heat by combustion, and gives a lower temperature in the flame, than carbonic oxide, still it is the best substance to be used for diluting the olefiant gas, since it affords no carbonic acid. On the contrary, marsh gas is worse even than carbonic oxide, on account of its producing much more heat and carbonic acid. In regard to the amount of carbonic acid generated, and the heating effect produced for a given quantity of light, gas, however, has a great advantage over any of the lighting materials formerly used. As compared with tallow, for instance, gas produces only one-half or less than half as much carbonic acid, and little more than one-third the heating effect. But it would be a very great improvement in the manufacture of gas if it could be produced with hydrogen alone as the diluting substance. Many years ago an attempt was made to effect this by Selligue, in Paris, and subsequently the same thing was tried in this country by Professor Donovan and Mr. White, under the name of water gas; but both projects proved unsuccessful, owing to a variety of adverse circumstances.

It will be evident from what has already been said with regard to the dependance of the luminosity of a flame upon its temperature, that it is desirable to produce the highest attainable temperature in the flame, and that any means of increasing that temperature would have the effect of increasing the illuminating effect. A very elegant contrivance for this purpose, in the case of gas, has been devised by Dr. Frankland. It consists in heating the air consumed in the combustion of the gas, by means of the waste heat radiated from the flame, by making it pass down along the side of the chimney before entering at the bottom of the burner to supply the flame. In this way the air is heated to 500° or 600° F., and there is, of course, a corresponding increase of temperature in the flame. Dr. Frankland has ascertained that by this simple appliance the same amount of light may be obtained with a saving of 49 per cent. of gas, and that for a given consumption of gas there is an increase in the illuminating effect to the extent of 67 per cent.

Returning now to the liquid hydro-carbon oils used for lamps, I will endeavor to point out how the principles I have described in reference to gas obtain in the application of those oils to lighting purposes. In this case the hydro-carbon vapor is not mixed with any diluting substance, as in gas, and all of these oils would give off a considerable amount of smoke if burnt in the same way as fat oils, by reason of the highly carbonaceous nature of their vapors. This circumstance was one of the most serious impediments to the introduction of these oils in the first instance, but eventually a lamp was devised which satisfied all requirements, and which is now largely in use. To understand the way in which the combustion of hydrocarbon oils in these lamps is effected without production of smoke, it must be remembered that in ordinary cases the flame of any lighting material, being for the most part gaseous and in contact with atmospheric air, there is, in accordance with the well-known laws of gaseous diffusion, a continual intermixture of air with the gaseous substances in the flame, and the extent to which this intermixture takes place is determined by several conditions, which may be regulated at pleasure. To take the simplest case, that of gas; the rate of intermixture will be proportionate with the rate at which the gas issues from the burner, and to the velocity of the current of air passing along the sides of the flame. When gas is burnt with a large Argand gas burner it is very liable to smoke, but when a glass cylinder is placed over the flame, the gas burns without smoke, in consequence of the greater intermixture of air with the flame, caused by the draft of the chimney. The degree of draft, and consequently of intermixture of air with the burning gas, will depend upon the height of the chimney. With a very high chimney and powerful draft the intermixture may be effected to such an extent as to neutralize, almost entirely, the illuminating power of the flame. In order, therefore, to effect perfect combustion without loss of lighting effect, it is necessary to proportion these determining conditions of the intermixture of air with the flame, according to the nature of the material to be burnt.

It is in this manner that the combustion of the hydro-carbon oil in lamps is effected. By means of the chimney a strong current of air is produced, and by means of the perforated cone, immediately over the wick, that current of air is made to impinge upon the ascending vapor, and mix it so as to enable it to burn without smoke. In this case, therefore, the air, by means of which combustion is supported, is made to serve the purpose of the diluting gases in ordinary coal gas, giving greater bulk to the gaseous contents of the flame, and effecting such a distribution of the ignited particles of carbon, as to admit of their being perfectly burnt.

The necessity of this admixture of air with the vapor of hydro-carbon oil to enable it to burn without smoke is of course greater in proportion as that vapor is denser and more highly carbonaceous. In this respect it will be seen that the several members of the series of substances constituting these oils differ materially. For instance, the vapor density of No. 16 is twice as great as that of No. 4, and there is the same difference between the amount of carbon in the vapors. The former vapor would also have double the illuminating power of the latter; but the size of the flame of No. 16 would be proportionately smaller, and owing to these two circumstances, the ignited particles of carbon would be so crowded together that a great deal of smoke would be produced unless a copious intermixture of air was effected by means of a very vigorous draft.

Practically the difference between the several substances here referred to, compensate each other in consequence of their being mixed together, in the various kinds of hydro-carbon oils. Thus the higher illuminating power of the denser vapors is to a great extent rendered available by the more bulky nature of the less carbonaceous vapors, and in this way a mean result is arrived at, of both characters combined, which answers all desired purposes. Everything depends upon the due proportionate mixture of the different substances.

From these considerations it will be seen that there is a positive disadvantage in carrying the separation of the more volatile portions of the oil beyond such a point as is indispensably requisite for its being used with safety; for the more those substances are separated from the oil intended to be used in lamps, so much the more must the least volatile portions, at the opposite extremity, be separated from it, in order to obtain a material fit to burn without smoke, and with a sufficiently large flame.

It will now be necessary to consider briefly the applicability for lighting purposes of the hydro-carbon oils which are too volatile to be used in the ordinary lamps. It is a fact that will be familiar to many of the members of this Society, that the idea of applying such highly volatile hydro-carbons as lighting materials is not, by any means, a thing of yesterday. It was first conceived, I believe, long before the year 1830, by a gentleman who has occupied, and still occupies, a most prominent position among those who have contributed to the successful establishment of gas-lighting. Mr. Lowe's original proposal was to use the volatile hydro-carbons obtained from the tar of gas works in the place of water in gas metres, so as to serve the double purpose of measuring the gas supplied to consumers, and of augmenting its illuminating power. Next to him came Professor Donovan, who proposed to make water-gas the medium for vaporizing these liquid hydro-carbons; and he appears to have formed this idea independently of any knowledge of Mr. Lowe's plan of naphthalizing gas. However that may be, the originality of the project in point of time certainly belongs to Mr. Lowe. These projects attracted very great attention at the time; but they never came into anything like general application. It may be that on reflection, the gas companies did not esteem very highly a plan for doubling the illuminating power of their gas, since that advantage was not necessarily accompanied by a demand for twice as much light as was previously used.

Some years afterwards another attempt was made to employ these highly volatile hydro-carbons as lighting materials, by the late Mr. Mansfield, to whom we are indebted for a very excellent investigation of the chemistry of gas-tar. His plan was to use atmospheric air as the medium for volatilizing the hydro-carbons, chiefly with the view of applying them in this manner for the lighting of country-houses or other places remote from gas-works. The material that he employed for this purpose was the more volatile portion of gas-tar, now known to chemists under the name of benzol; it possessed a character, however, which proved fatal to the undertaking—it was that of becoming solid when considerably cooled. In consequence of this the reduction of temperature produced by its own volatilization rendered it solid, and stopped its further evaporation.

Strangely enough, this very same plan of using atmospheric air saturated with the vapor of a volatile hydro-carbon has quite recently been put forward with much pretence, as a totally new invention, notwithstanding the fact that Mansfield's patent entirely covers this application of these substances, and that as this patent has expired, the process is now public property.

The naphthalizing of coal-gas, though still older, has also been brought before the public lately, under a new name, as being a new invention.

It now only remains to consider how far it may be practicable to use the highly volatile hydro-carbons of petroleum and similar materials for lighting purposes, by diffusing their vapor through atmospheric air. These liquids are free from that prejudicial character of solidifying when cooled, which renders benzol obtained from gas-tar, inapplicable for the same purpose. They are now obtained in tolerable abundance in connexion with the manufacture of hydro-carbon lamp oils, and they are to a great extent still mere waste products, since they do not answer well as a substitute for turpentine, which is the chief use they have yet been put to. It is, therefore, possible that these liquids might be obtained in sufficient quantity and at such cost as would admit of their being used as lighting materials.

By a reference to the table it will be seen that, as compared with olefiant gas, their illuminating power is considerable. Taking the substance No. 7 as representing the available volatile portion of petroleum, its vapor has 3½ times the illuminating power of olefiant gas; in other words, gas containing 3½ per cent. by measure of this vapor would have the same illuminating power as coal-gas containing about 12 per cent. of olefiant gas, and with 7 per cent. of this vapor it would be equal to the best cannel gas.

There is one important point, however, in regard to this application of the volatile hydro-carbons which cannot be overlooked, and which requires to be thoroughly examined before any attempt is made to use them in this way. It is well known that hydro-carbon gases, such as olefiant gas or marsh gas, the representative of all these hydro-carbons, when mixed with air in certain proportions, form a mixture that is violently explosive. It, therefore, becomes a question of very serious moment whether this fact presents any real or insurmountable obstacle to the use of atmospheric air as the medium for using these volatile oils in the condition of gas—whether, in fact, under the conditions to be observed for such a use of these oils it would be possible to get a mixture of the vapor and of air that would be explosive when brought in contact with a flame. If that be possible, even in some extreme case, that may be of unlikely occurrence, then there can be no doubt that the use of these oils in that way would be highly objectionable.

It cannot be doubted, looking at the mere possibility of the matter, that with these vapors, just as with marsh gas, an explosive mixture might be produced. There is no question as to that. The question really to be considered is, whether such a result could take place under the conditions for using these oils.

It is well-known that the explosibility of a mixture of marsh gas and air depends upon the relative proportion of the gas and air. For its perfect combustion marsh gas requires 10 times its volume of air. Such a mixture is explosive when brought in contact with flame, and the explosive character still continues when the proportion of air is not more than six or seven times as great as the marsh gas. But when marsh gas is mixed with only three or four times its volume of air, the mixture is not at all explosive, but will burn like the unmixed gas. In the same manner, other hydro-carbon gases require to be mixed with a certain proportion of air to form explosive mixtures; and with regard to the vapors of the volatile hydro-carbons, this is equally the case. In consequence of the density of these vapors being so much greater than that of their representative—marsh gas—they require much larger proportions of air for their combustion. Thus for instance, the vapor of the oil No. 7 requires 70 times its volume of air for combustion. In all probability such a mixture would be explosive. But a mixture of this vapor with air which would possess an illuminating power equal to cannel gas, would not contain more than 27 parts of air to one of the vapor. Such a mixture, I believe, would not be explosive under any circumstances that require to be taken into account.

Then as regards the possibility of an explosive mixture being formed, it appears to me highly probable that the extreme volatility of these liquids would in itself constitute a most effectual safeguard against the formation of an explosive mixture. To ensure the efficacy of this character of the oils, however, it would be indispensable that arrangements should be made for securing the intimate contact of the air with the hydro-carbon to be volatilized, and likewise the constant maintenance of a fresh supply of hydro-carbon. All these points, however, are mere matters of detail, which would acquire importance only after the determination of the fact that, with a sufficiently volatile oil, its vapor mixed with air might be burnt like gas without any danger of an explosive mixture being formed. If that were satisfactorily proved to be the case, it is likely that this mode of using those portions of hydro-carbon oils that are still without any satisfactory application, might be introduced with advantage in many cases. Among others I may mention the lighting of railway carriages, and of houses or public institutions situated at a distance from gas-works.

(To be Continued.)

For the Journal of the Franklin Institute.

On Dr. Brünnow's Magnetic Break Circuit.

By S. W. Robinson, C.E.

AN IMPROVEMENT.

In No. 19 of the "*Astronomical Notices,*" edited by the able mathematician and astronomer, Dr. Francis Brünnow, who, till recently, held the position of Professor of Astronomy and Director of the Detroit Observatory, at Ann Arbor, Michigan, is described a new break-circuit invented by himself, and which was adopted at that Observatory, where it has been proved a success. It was also used by him at the Dudley Observatory, while director there.

In entering upon his description he says:

"The contrivances used for breaking the circuit of a chronograph by the pendulum of a clock every second, as far as they are known to me, alter the clock rate considerably wherever they are immediately applied to the pendulum. When this is not the case, as in Mr. Vintle's method, described in No. 1153 of the *Astr. Nachrichten*, or in Mr. R. Bond's new escapement, the clock requires some alterations which it may not always be convenient to introduce. Trying, therefore, to find a contrivance which might be easily applied to the pendulum of any clock without disturbing its uniform rate, I hit upon the idea of using the attractive force of a small magnet connected with the pendulum; and the experiments which I have made with such an apparatus have been so successful that I do not hesitate to recommend the method."

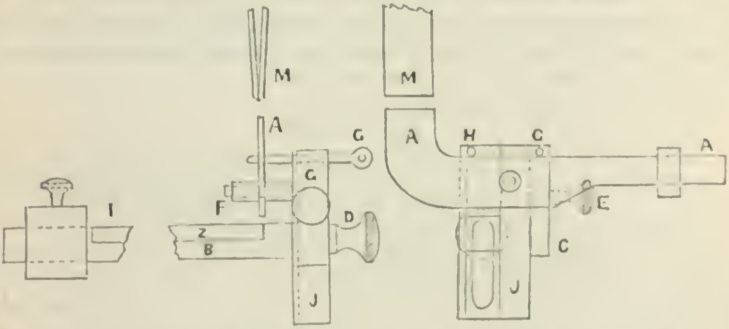
As adopted by him, it consists essentially of a small bar magnet attached to the lower extremity of the pendulum, in a position perpendicular to its plane of vibration, and a small armature arranged in such a manner as to be attracted by one end of the magnet bar as it passes with the vibration of the pendulum. When the armature is attracted, a platinum point, resting against a screw point of platinum, is disconnected, which breaks the circuit of the battery.

The chief objection to this arrangement of the magnet and armature is the liability of the pendulum to move in a slightly elliptical orbit, the end of the magnet bar passing nearer the armature in one vibration than in the other. This tendency was chiefly obviated by placing a stationary armature opposite the first. But unless in the best adjustment, difficulty was still experienced in the striking of the magnet.

I have improved this arrangement by simply attaching the bar magnet in a vertical position, directly under which is the armature whose motion breaks the circuit. With this no difficulty can possibly arise from a deviation of the pendulum from its plane of vibration.

It is unknown to me whether the method occurred to Dr. Brünnow earlier than the year 1859, but as far as I have been able to obtain information he was the first who tried it *successfully* and made his results known to the public. At about the same time, Professor C. A. Young, of the Western Reserve College, hit upon the same idea; but after a brief trial with a rude apparatus, pursued the subject no further.

Having been brought in connexion with the use of the astronomical clock and chronograph on the Survey of the North and North-west Lakes, under the superintendence of Colonel Wm. F. Raynolds, A.D.C., and Maj. Engrs., for which clock Mr. Saxton's break-circuit was used,[*] which seemed to change the rate of the clock considerably for every adjustment of the mercurial connexion, I suggested Dr. Brünnow's method, with the exception of arranging the magnet and armature as described below. After a trial apparatus had been tested, the method was adopted, and a well-finished one for each of the two clocks ordered according to my drawing, an exact copy of which is given here.



M is the bar magnet about two inches in length, composed of two thin pieces of steel plate well magnetized. One is placed[†] upon each side of the index point of the pendulum, and secured by a small clamp not represented. This attachment is so simple that it can be made to any ordinary clock. A is the armature, or rather a thin steel magnet in itself, presenting opposite polarity to the magnet M. This is carried upon a tube F of brass, or better, platinum, and the whole supported and movable upon a knife-edge of brass or platinum passing through F. There should be a sharp corner in the upper side of the tube for the knife-edge, to prevent the swaying of the armature. H and G are points which limit the motion of the armature. G is supported by an ivory piece C to insulate it, having a slight vertical motion, and secured by E. With this the motion of A is regulated.

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[*] With the exception that the tilt hammer plays into a cup of mercury instead of striking a platinum surface. With a battery of three cups, the mercurial contact oxidizes so rapidly as to become necessary to raise the hammer and remove the surface of the mercury several times during a night's observations. In doing so, the hammer is usually raised higher than its motion from the clock. Either from a springing of the wire, or a changed condition of the mercury surface, or both, it is believed that the clock rate has been disturbed.

[†] North polarity should be placed downward for the better retention of the magnetism.

The bearing of the armature against G should be platinum. The brass piece J has a vertical motion for adjusting the distance of the armature from the magnet, and secured by D. B is a bar, simple or compound, connecting the apparatus with the stand I. If the clock has a gridiron pendulum, the simple bar only is necessary; but if the pendulum is compensated with mercury, its lower extremity rises and falls, from the influence of heat, and in order that the distance between the magnet and armature may be constant, I adopted a compound bar, B Z, of brass and zinc, avoiding the use of iron, for the influence it would exert upon the magnet bar M. In order to know the length of the zinc insertion Z, I found the following interesting problem presented to me, viz: *to determine the deflection of a compound bar from the influence of heat.*

I intend to offer more upon this at some future time. My space here is too limited for a full discussion of the subject. I will, however, give the formula with which I calculated the radius of curvature of the bar. This is constant for the whole length of the bar, for a given degree of temperature, and a bar of uniform size. Hence the curve is a circle, and the length can be calculated for a given deflection by a substitution in the equation of the circle.

Let d = the uniform depth of the bar,
b = the uniform breadth,
E and E' the moduli of elasticity of the two metals, assuming them the same for elongation or compression,
e and e' the co-efficients of expansibility,
ρ = the radius of curvature,
and n = the change of temperature.

Then I get

$$\rho = \frac{d}{2n\,(e-e')}\left(1 + \frac{E+E'}{2\sqrt{(3\,E\,E')}}\right)$$

What can be required of a break-circuit further than that the rate of the clock should be unaffected by it? its easy applicability to any clock; a constancy in its operations; that it should produce short breaks; and its unliability to get out of repair. All these qualities are possessed in a high degree by this break-circuit.

This break-circuit is now in use upon the Survey of the North and North-west Lakes, under the direction of our able and esteemed superintendent, to whom it is yielding the utmost satisfaction, and to whom reference can be had concerning it.

I append here some of Dr. Brünnow's results:

"When a chronograph is used with one writing pen only, it is important to have the breaks very short. With my apparatus I succeeded in making breaks of 0·06 of a second, and as this can never produce a greater error in reading an observation coinciding with a second break, than 0·03 of a second, it is sufficient for all practical purposes.

"My clock before the magnet was attached, and the break-circuit before it, was regulated to sidereal time. With the magnet and break-circuit the observed errors of the clock have been as follows:—

			Daily Rate.
May 3	10h	+26·10	
			—1·28
4	10	24·82	
			—1·25
5	12.2	23·46	
			—1·25
11	13	15·94	
			—1·27
15	14	10·81	
			—1·20
22	12	2·47	
			—1·17
23	12	+ 1·30	
			—1·26
25	12	—1·22	
			—1·32
29	12	6·52	
			—1·32
June 2	14	—11·90	

"A better rate than this can hardly be expected, and shows that the magnet has no sensible disturbing influence."

Barometers.

From the Journal of the Society of Arts, No. 601.

Sir: In your *Journal* of the 1st of April appears a description of a so-called new barometer, which, I think, will be found accurately described in the following extract from the *Encyclopædia Britannica*, 7th edition, 1842, and written by Sir J. Leslie:—"The most accurate construction of a barometer of this kind [the conical barometer, invented by Amontons, 1695,] is attained by joining together two tubes that have even but unequal bores, the longer and narrower being uppermost. If the width of the upper tube were supposed to be, to that of the under one, as two to three, the scale would be enlarged three times, since, by descending three inches from the top, and, consequently, two at the bottom, the column would suffer a contraction of one inch in height. This species of barometer is thus recommended by its simplicity and its ample range; but the bore of the tube being indispensably small, the mercury moves with difficulty, and resists impressions of minute changes of external action." Also, page 400:—"A modification of the conical barometer in traveling we have ourselves employed. The principal part consists of a small steel stop-cock, a glass tube 31 or 32 inches long, with a bore of the tenth of an inch, sealed at top and filled with quicksilver, is cemented into the one end of the stop-cock, and into the other end is cemented an open and wider tube, 16 inches or more in length, and having its diameter above the eighth of an inch. This compound tube is lodged in a walking-stick, divided into inches and tenths through its whole extent, or only at the upper part." It will thus been seen that this barometer was invented and published more than 20 years since, and, if necessary, I could mention persons, both in London and elsewhere, who have made them, but probably they have not been much used, from the defect alluded to, which cannot be got rid of, except by using a larger tube, and having a movable piston in the bottom, on Whiting's plan, as exhibited by Mr. Becker last year at the soirée of the Royal Society. Mr. Becker informed me, and, no doubt, others as well, that Whiting had also made them, as described above, with the small tube. Mr. Becker's instrument led, no doubt, to the recent revival of this kind of barometer; but while, perhaps, as a matter of business, it may be thought legitimate for any maker to introduce, in connexion with his own name, an instrument not generally known in the trade, it was hardly to be anticipated that the Royal Society should lend the sanction of their authority to the introduction of an instrument as new which was published more than 20 years since in a by no means obscure work. The above quotations appear also in the eighth edition of the *Encyclopædia.*

I am, &c., W. SYMONS.

Constancy of the Daniel's Battery.

From the London Chemical News, No. 242.

In order to increase still more the constancy of the Daniel's battery, Father Secchi advises the use of fine sand or of powdered sulphur in the porous cell. He accounts for its action by supposing that when the ordinary liquid alone is used there is greater liability to local action taking place upon the zinc. In a battery, the circuit of which is closed for two minutes every quarter of an hour, the learned Father has used an ordinary piece of commercial sheet zinc, half a millimetre in thickness, which has continued in action for more than six months, without showing the least sign of corrosion. For large elements instead of porous diaphragms, he uses bags made of coarse linen cloth, well annointed with a luting of flour and lime. Those who have experienced the difficulty of procuring large porous cells would do well to test these contrivances.

A Case of Dialysation.

To the Editor of the London Chemical News.

Sir:—In preparing solutions of vegetable acids in a large manufactory lately, where, as usual, the solutions are separated from an insoluble residue which is washed until tasteless, I found in one of the filters an impossibility of accomplishing this, the acid taste remaining, although water freely percolated through the mass; and on closer examination I found that the filtered liquor, which originally contained only a very small percentage of sulphuric acid, now consisted almost entirely of it, the crystallizable acid having been kept back by the precipitate, which in this case seems to have acted as a dialyser. I may further add that as soon as there was more space allowed below the false bottom, so that the liquor could not remain at all in contact with the precipitate, this effect ceased.—I am, &c., CHEMICUS.

A Comparison of some of the Meteorological Phenomena of AUGUST, 1864, *with those of* AUGUST, 1863, *and of the same month for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	August, 1864.	August, 1863.	August, for 14 years.
Thermometer—Highest—degree, .	96·00°	95·00°	97·00
" " date, .	11th.	10th.	2,'56; 4, '59.
" Warmest day—Mean,	87·50	88·50	88·50
" " " date,	11th.	10th.	10th 1863.
" Lowest—degree, .	61·00	58·00	47·00
" " date, .	31st.	30th.	26th 1856.
" Coldest day—Mean,	68·83	63·00	59·00
" " " date, .	31st.	30th.	26th 1856.
" Mean daily oscillation,	11·71	16·00	15·75
" " " range, .	2·73	3.79	3·72
" Means at 7 A.M., .	75·26	74·95	70·96
" " 2 P.M., .	81·19	85·50	81·39
" " 9 P.M., .	78·43	78·05	74·03
" " for the month,	79·29	79·50	75·46
Barometer—Highest—Inches, .	29·938 in.	30·119 in.	30·255 in.
" " date, . .	19th.	31st.	20th, 1855.
" Greatest mean daily press.	29·915	30·107	30·229
" " date, . .	20th.	31st.	20 & 31 1855.
" Lowest—Inches, .	29·437	29·699	29·356
" " date, . .	3d.	29th.	20th, 1856.
" Least mean daily press.,	29·456	29·762	29·388
" " date, . .	3d.	29th.	20th, 1856.
" Mean daily range, .	0·082	0·100	0·095
" Means at 7 A.M., .	29·726	29·878	29·867
" " 2 P.M., .	29·694	29·850	29·839
" " 9 P.M., .	29·726	29·868	29·858
" " for the month, .	29·715	29·865	29·855
Force of Vapor—Greatest—Inches,	0·895 in.	0·980 in.	1·024 in
" " " date, .	2d.	10th.	1st, 1854.
" " Least—Inches, .	·307	·268	·268
" " " date, .	31st.	30th.	often
" " Means at 7 A.M., .	·633	·585	·587
" " " 2 P.M., .	·630	·590	·594
" " " 9 P.M., .	·673	·610	·615
" " " for the month,	·645	·595	·599
Relative Humidity—Greatest—per ct.,	90·0 per c.	88·0 per c.	100·0 per ct.
" " " date,	21st.	16th.	26th. 1854.
" " Least—per ct.,	38·0	41·0	27·0
" " " date, .	31st.	30th.	1st. 1860
" " Means at 7 A.M.,	71·3	76·8	76·4
" " " 2 P.M.,	53·3	60·0	56·0
" " " 9 P.M.,	68·7	75·0	73.2
" " "for the month	64·4	70·6	68·5
Clouds—Number of clear days,* .	4 days,	10·	9·6
" " cloudy days, .	27 "	21·	21·4
" Means of sky cov'd at 7 A.M.,	70·6 per ct.	55·0 per ct.	55·7 per ct.
" " " " 2 P.M.,	77·4	60·0	60·9
" " " " 9 P.M.,	58·4	45·0	43·1
" " " for the month.	68·8	53·3	53·2
Rain.—Amount,	1·539 in.	1.440 in.	3·889 in.
No. of days on which Rain fell,	9·	9·	9·9
Prevailing Winds—Times in 1000	s 42°39′w ·298	n 84°17′w 175	s 75° 43′w. 102

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* Sky one-third or less covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the SUMMER *of* 1864, *with that of* 1863, *and of the same Season for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	Summer, 1864.	Summer, 1863.	Summer, for 13 years.
Thermometer—Highest—degree,	96·00°	95·00°	100·5°
“ “ date,	26Je & 11 Ag	10th Aug.	21st July, '54
“ Warmest day—Mean,	89·67	88· 50	91·30
“ “ “ date,	26th June	10th Aug.	21st July, '54
“ Lowest—degree,	51·00	52·00	42·00
“ “ date,	12th June.	8th June.	5th June, '59
“ Coldest day—Mean,	59·33	60·83	55·00
“ “ “ date,	11th June.	7th June.	6th June, '61
“ Mean daily oscillation,	14·95	14·28	16·14
“ “ “ range,	4·08	4·06	4·14
“ Means at 7 A. M.,	71·66	71·71	71·04
“ “ 2 P. M.,	81·97	80·16	81·19
“ “ 9 P. M.,	75·14	74·09	73·74
“ “ for the Summer,	76·26	75·32	75·32
Barometer—Highest—Inches,	30·087 in.	30·119 in.	30·281 in.
“ “ date,	21st June.	31st Aug.	14th June, '52
“ Greatest mean daily press.	30·038	30·107	30·251
“ “ date,	18th July.	31st Aug.	13th June, '52
“ Lowest—Inches,	29·296	29·321	29·182
“ “ date,	9th June.	1st June.	11th June, '57
“ Least mean daily press.,	29·358	29·377	29·262
“ “ date,	9th June.	1st June.	11th June, '57
“ Mean daily range,	0·108	0·087	0·096
“ Means at 7 A. M.,	29·774	29·807	29·839
“ “ 2 P. M.,	29·739	29·781	29·809
“ “ 9 P. M.,	29·771	29·810	29·826
“ “ for the Summer,	29·761	29·799	29·825
Force of Vapor—Greatest—Inches,	0·895 in.	0·980 in.	1·059 in.
“ “ “ date,	2d. Aug.	10th Aug.	30th June, '55
“ “ Least—Inches,	·221	·207	·142
“ “ “ date,	28th June.	16th June.	14th June, '61
“ “ Means at 7 A. M.,	·542	·561	·567
“ “ “ 2 P. M.,	·547	·561	·574
“ “ “ 9 P. M.,	·589	·580	·597
“ “ “ for the Summer,	·559	·567	·579
Relative Humidity—Greatest—per ct.,	97·0 per ct.	94·0 per ct.	100·0 per ct.
“ “ “ date,	25th July.	22 & 29 June.	26 Au. '54. 6 Je.'56
“ “ Least—per ct.,	24·0	22·0	22·0
“ “ “ date,	28th June.	16th June.	16th June, '63
“ “ Means at 7 A.M.,	68·1 per ct.	74·1	73·2
“ “ “ 2 P.M.,	49.4	58·5	54·0
“ “ “ 9 P.M.,	66·3	71·9	71·1
“ “ “ for Summer,	61·3	68·2	66·1
Clouds—Number of clear days,*	22	16	25·5
“ “ cloudy days,	70	76	66·5
“ Means of sky cov'd at 7 A.M.	60·7 per ct.	71·6 per ct.	58·6 per ct.
“ “ “ “ 2 P.M.	63·6	69·0	60·6
“ “ “ “ 9 P.M.	46·7	47·3	43·3
“ “ “ for Summer,	57·0	62·6	54·2
Rain—Amount,	7·527 in.	11·183 in.	12·045 in.
No. of days on which Rain fell,	24	39·	32·8
Prevailing Winds—Times in 1000,	s63°49w′·290	s63°26′w·080	s71°25′w· 160

* Sky, one-third or less covered at the hours of observation.

JOURNAL

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FOR THE

PROMOTION OF THE MECHANIC ARTS.

OCTOBER, 1864.

CIVIL ENGINEERING.

On the Testing of Chain Cables. By FREDERICK ARTHUR PAGET, Esq., C.E.

From the Journal of the Society of Arts, No. 598.

(Continued from page 152.)

THE question as to the re-testing of cables that have been in use for a certain time is yet unsettled, but the inquiry is of scarcely less importance than that of the first testing. There are many applications of wrought iron in which it is subjected to impulsive stresses, often more or less accompanied by vibrations, and in which, nevertheless, the detail or structure has to conform to certain narrow limits of size and weight. Such is the case with most applications of chains; for instance, to cranes, inclines, forge-slings, &c. Such is the case also, more or less, with railway axles; the axles of carriages on rough common roads; the gags of helve hammers; the porter bars fixed to the blooms whilst under the hammer; the iron wires of some piano-fortes; and many similar applications of wrought iron. The simple fact that only one-half of the gradually applied stress required to produce the proof strain will, if applied suddenly, of itself produce the proof strain (which if exceeded would injure the piece), goes a long way in explaining the matter. Where great interests of life and property are involved in the safe action of these applications of iron, the irresistible logic of facts has sometimes caused preparatory allowances to be made for these "fatigues of the metal." The axles of the London omnibuses are stated to be always renewed after having run a certain fixed mileage. This system is also carried out with the carriages of the Messageries Générales, the axles of which are changed after having run a limit of 40,000 kilométres. The Honorable the Corporation of the Trinity House entirely renews all the moorings of the light-ships every four years—one-fourth of the number yearly. This limit of time gives the measure of the perfect efficiency of a good cable, well proportioned to its work, and in constant use day and night. Cables in ordinary ships are of course much less, or rather much more slowly, subject to deterioration. We have seen that M. David fixed the time after which a cable in ordinary use should be tested at ten or twelve years. Mr. Macdonald, of the Liverpool testing house, stated, before the 1860 Committee, that he would examine a cable after any long voyage—such as to India or Australia. The late Mr. Green, the great shipowner, explained that this was done with the mooring tackle of all his ships. An experienced pilot, Mr. G. J. Thompson, said that it should be made imperative to re-test chain cables every six years, and Mr. Smale fixed this limit at seven years. Mr. J. R. Clarke, however, the chief clerk of the store office, stated that there were many sound cables in store twenty years old. It is clear that it would be very difficult to fix a limit of time that could be applied to all classes of ships. The cables in the royal ships are scarcely so often or so severely tried by use as those of some merchant vessels. A cable might remain good for many years, and yet at last be injured in a single storm. Apart from accidents, such as abrasion on rocks, or against a sharp-cornered anchor stock, or similar causes, there are three main conditions affecting the duration of cables and furthering their progressive deterioration under wear:—1st, the friction and abrasion at the crowns; 2d, rust and corrosion by the sea-water; 3d, undue strains on the cable, and in excess of the compressive and tensile elastic limits of the materials. The average amount of abrasion and consequent wear at the crowns could only be determined by a statistical comparison of the deterioration of a number of cables, worked under similar circumstances, through a certain period of time. No full observation of this kind seems to have been yet made. The same appears to be the case with the deterioration of iron cables by rust and corrosion. Mr. Mallet has observed, "that the metallic destruction by corrosion of iron in sea-water is a maximum in clear sea-water of the temperature of 115° Fahr., that it is nearly as great in foul sea-water, and is a minimum in clear fresh river-water." It also appears that, the finer and more equable the quality of the iron, the slower is its corrosion. The alternative action of the air and the sea-water in ordinary cables must have some influence on their deterioration. Again, at a depth of, say, 100 fathoms, there would be a pressure of nearly 17 tons on the square foot, and this pressure would search out any slight crevice, or any slightly defective weld that had escaped the test. It is at these places that the corrosive action of the water is most felt. It is a well ascertained fact that the spongy mass of mechanically compressed crystals we call wrought iron, is porous, as water can be forced through it at comparatively moderate pressures. It is also well known that the hydrated oxide of iron we term rust performs the part of an electro-negative element when in contact with metallic iron, which is then electro-positive. When iron is rusting in the air, the moisture of the atmosphere is the exciting liquid, but this voltaic action must be greatly intensified in the presence of sea-water. I have noticed the interesting fact—which deserves more investigation than I have yet been able to give to it—that in the links of a great number of chains the wrought iron is much more eaten away at the sides, where it is in contact with the cast iron cross-stay. The same action was stated, in a number of the *Times* of last year, to have been observed on wrought iron tie-rods in contact with the plates of a cast iron sea-water tank which burst last June at Woolwich. I had lately occasion to examine a number of old chains, after they had been cleaned, and after the rust had been knocked off with a hammer. All the cast iron cross-stays, almost without an exception, were slack. Each link was thus temporarily reduced to the condition of an unstayed link, the ultimate strength of which, compared with a stayed link, is generally taken to be in the ratio of 7 to 9. When the cable is in use, the progress of this undoubtedly voltaic action in weakening them will be aided by mechanical causes. The rust generated between the cross-stay and the sides of the link will be more or less washed out by the surge of the cable; a sufficient longitudinal stress would cause the virtually unstayed link to collapse on the stay; the sea-water would again search out the chinks; would again decompose the material; and the deterioration of the cable thus chemically and mechanically weakened, would progressively advance in successive increments that would render its ultimate destruction a mere matter of time. This action would be, of course, more felt in a cable in constant use, such as those of the Honorable the Corporation of the Trinity House; and whether zincing, which is stated by Dr. Parry to prevent rust, would be of any use, or whether other means, which will doubtless occur to many here, might prevent, or at least modify, this action, is perhaps, a question worthy of investigation by the able men comprising the Trinity Board. There is, however, no need to search amongst the mysterious forces of nature for the main cause that leads to the ultimate destruction of a cable, or of any other application of iron, under like conditions. The primary cause of the destruction of a cable is simply due to the limit of elasticity of its material being exceeded. All chains are, by their very structure and special uses, subject to jerks and shocks; any country blacksmith knows that a chain that can stand a dead pull, would give way under the same weight if suddenly applied; and we all know that a careless laborer at the winch handle of a crane sometimes breaks down a good chain by a heedless jerk. Little more than 5½ tons to the square in. if suddenly applied, would at once bring on the proof strain of 11·46 tons; and although the dead weight of a cable is its greatest safeguard—so much so, in fact, that if the cable out of the hawser could be weighed at different parts of its length, this would be an advantage—yet, nevertheless, the safe load of about 2¾ tons, under an impulsive stress, to the square inch, must be often exceeded in practice. The safe load under an impulsive stress is in truth rather less, as the assumption is based upon the usual notion, which assimilates a cross-stayed link to a couple of bars.

It appears a paradox to say that the chain is, in one sense, strengthened by a strain in excess of the elastic limit, but such is the fact. The power to bear a static load is indeed increased, as was shown by the experiments cited, before the Committee of 1860, to prove that a cable is strengthened by being broken several times under the gradually applied load of the hydraulic press; and, as was also shown by the performances of the $1\frac{5}{8}$th bars subjected to the same treatment by Mr. Loyd. The link is, in the first place, mechanically strengthened by being drawn into a lozenge-like shape, as the two sides of each end then act as ties to a very short beam; but this is obtained at the expense of the elasticity of the material—the material of the link is rendered harder. It is a somewhat fanciful analogy to compare the limits of elasticity and of rupture of iron to the organic life of a plant or animal, but it is justified by the common expression that a bar is said to be crippled by an undue strain. If this living force in a bar—these *forces vives de resistance*, as they are termed by Poncelet—if, in one word, the work to be done in stretching a bar be expressed (in the English way shown by Mr. Mallet) by multiplying half the static load in pounds required to stretch a bar one foot long and of one inch cross-section to its limit of elasticity, by its elongation in terms of a foot (T_e); and if the static load required to break the bar be expressed in the same way—by multiplying half the static load in pounds by the ultimate elongation in terms of a foot (T_r)—we shall then get the power for work expressed in foot-pounds, or the structural value of our bar, and shall see the reason that a chain may be crippled for any application in which it is subject to an impulsive force. The short range, multiplied into the high static load required to stretch a bar of hard iron to its limit of elasticity, compared with the product of the long range but low static load required to stretch a bar of soft, ductile iron, will show that a link made of hard, brittle iron will keep its shape much better than one made of soft, ductile iron. A calculation of the work done in rupturing a bar of soft iron will show that its living force of resistance to rupture is several hundred times greater than the force required to alter its elasticity; and a similar calculation of the work done in rupturing a bar of hard iron will show that the work to be done in breaking it is perhaps twenty times less than that in stretching it to its elastic limit. As any impulsive force is equal to twice the work to be done in producing or consuming it, and as the effort required is less as the distance gone over is greater, it will be seen that, although resilience is a *sine quâ non* in a cable, the strength of the links would be destroyed, and the structural flexibility of the whole cable would be injured, by the use of iron too soft; while the use of very hard iron in the first instance, or the ultimate hardening of any iron when its limits of elasticity are exceeded, renders a cable of hard or hardened iron utterly useless for its intended purpose.

There is thus no necessity to have recourse to any theory of the crystallization of iron under impulsive stresses to explain the gradual deterioration of a cable; but this question of crystallization is one of the greatest importance and interest; and we may yet learn that the structural value, for many purposes, of a given bar of iron is in some determinate relation to the size of the facets of the crystals of which it is composed. A good cable bar consists of crystals that have been more or less elongated while passing through the rolls; the question is whether these crystals are loosened or separated at their planes of cleavage, or whether the crystalline axes have been transposed, under the undue strains, more or less accompanied by vibration, to which chains in general, and chain cables in particular, are necessarily subject. There is no well-ascertained instance of any alteration of this kind happening under moderate stresses, but Mr. Mallet appears to believe that a reversal of the crystalline axes takes place when the elastic limit either of extension or compression, and therefore of flexure, is exceeded, and more especially if the piece be not initially in a state of molecular repose. There is every reason to believe in the existence of internal strains in the link of a chain, and more especially at the crowns. But numberless experiments by Dr. Rankine and others, and more particularly by Mr. Kirkaldy, have shown that what is popularly called a crystalline fracture may be given to the most fibrous piece of iron if it be broken under a suddenly-applied load—an effect simply due to the mechanical effect of a sudden stress, and to the fact that any piece of iron is an assemblage of crystals. There is no reason to believe that a magnifying glass—as was, indeed, shown by Robert Stephenson—would reveal any material difference between a bar broken after fatigue of whatever kind, or a bar broken when fresh from the mill. At the same time, the application of a very powerful miscroscope to the molecular structure of iron has yet to be made; and the history of the first application of the telescope to a very different science may yet find its counterpart in this department of physical knowledge.

Whatever be the internal effect of the lateral contraction induced by excessive tensile strains, it would be of the utmost importance to settle, once and for all, whether re-annealing can restore the living force of resistance of iron, and, therefore, of a cable. Mr. T. M. Gladstone, C.E., recommended this plan before the Committee of 1860. Mr. Smale, then of Woolwich, said that this would be like Burnetizing rotton wood. Dr. Noad, in a letter to the *Times*, about eight years ago, stated that he had taken away the brittleness of an old chain by keeping it for 24 hours in a furnace. The late Mr. Glynn recommended that a crane chain should be annealed every three years. At the North Roskear mine, in Cornwall, it is stated by M. Moissonet, that the pit-chains are withdrawn from the shaft after every six months use, are rolled in a heap, then covered with a sort of cylindrical furnace, and brought to a red heat. According to an account translated from the Russian into the *Polytechnisches Centralblatt*, the chain cables for the Russian government, after being brought to a dark-red heat immediately after testing, are then tarred—a plan which is said to prevent rusting, as the tar thus takes a firmer hold on the iron. But many things may be done with charcoal iron that it would not be safe to attempt with our ordinary iron. Baudrimont appears to believe that all metals only acquire determinate qualities by proper annealing, and that a cherry-red heat is necessary for annealing wrought iron. According to the experiments by the Franklin Institute, wrought iron is perfectly annealed at a clear bright red. The experiments of both Baudrimont and the Franklin Institute show that the ultimate tenacity of iron is considerably diminished by annealing, but, unfortunately, in neither case was the elongation noticed. Poncelet has shown that his co-efficient, T_e, of elasticity is increased with annealed iron, but that the co-efficient of rupture, T_r, is diminished. This refers to wires, and no complete experiments appear to have been yet made on the effect of annealing on bars. It is a question whether the extra ductility conferred on the links by the process of annealing would not, while rendering them more ductile, at the same time lead to their changing their form. At any rate, at least some of the cast iron cross-stays would be rendered less able to withstand distortion. At the same time, the question ought to be settled, and to cables comparatively uninjured by corrosion, the process might prove of great value. The conditions of size in a cable are peculiarly favorable to the use of annealing. Great as the advantage would be in the successful application of annealing to large forgings, there are several well-authenticated instances of massive crystals being developed in the interior of the mass by the long-continued action of a red heat. General Morin thus mentions an instance of the production of crystals, with facets from 4 to 5 millimetres in breadth, in a charcoal iron bar originally of fine, soft, fibrous, texture.

Tied as we are in testing cables within a narrow limit, which if exceeded in either direction would, on the one hand, either impair the efficiency of the cable, or on the other, the efficiency of the test, it is clear that the most thorough accuracy is required in measuring the proof stress. Unfortunately, it is not always the case that this accuracy is obtained. The stress exerted by the machine of M. David, of Havre, was shown by the French government to be taken too high. The appliance for the measurement of the stress exerted by the Liverpool corporation testing machine, was a few years ago shown by Mr. Mallet to give a result of nearly 9½ per cent. error in excess. Some of these machines consist of a powerful windlass purchase, but we will confine our attention to the direct-acting hydraulic press, the application of which to the testing of chain cables, by the late Sir Samuel Brown, may be said to have rendered the iron cable a practicable thing. There are three distinct ways of measuring, or at least approximately measuring, the stress exerted by the press plunger. 1st. A small valve is fitted to the cylinder and furnished with a steel-yard and adjustable weight. In large machines this is, for the sake of convenience, carried to a distance from the press, the water being conveyed in a small pipe. 2d. A Bourdon gauge is attached in the same way, either direct on the cylinder, or it is placed in communication therewith by means of a small pipe. 3d. The other end of the chain being tested is attached to the head of a bent iron lever, the power of which is multiplied by a system of levers balanced on knife edges. The plan of measuring the stress exerted by the press plunger, by means of a weighted valve, is liable to several objections, as was pointed out many years ago by Professor Peter Barlow, more recently by Professor Rankine, and by Mr. Bowman in his evidence before the 1860 Committee. In the first place, the relative proportion between the pump plunger and the valve is necessarily great; and a simple calculation will show that a hair's breadth more or less to the valve would make an important difference. In the next place, the friction of the leathers and the weight of the plunger are not taken into account; the gross load on the plunger is, in fact, given as the useful work at the end of the piston rod. Some experiments made by Professor Rankine, whose name is a sufficient guarantee in matters of this kind, have shown that about one-tenth should be deducted from the pressure in the hydraulic press, merely for the friction of the press plunger. The real, the useful work exerted at the end of the plunger on the chain is thus more than 10 per cent. less than is given by the pressure of the water. An error in the opposite direction will be made by conducting the pressure of the water, either on a weighted valve, or on a Bourdon gauge, and this error will vary with the diameter of the pipe, the number of bends, and the other losses of effect in a stream of water passing through a pipe, which are well known to engineers. The load on a safety valve is always an unreliable datum for computing pressure; a Bourdon gauge is much more delicate, but, in this case, its indications are erroneous, unless proper allowance be made for the friction of the leathers and the weight of the plunger. The most exact means yet employed for measuring the stress created by the plunger on the chain, consists in the use of a system of balanced levers, according to the plan adopted at H.M. Woolwich and Portsmouth dockyards, and by Messrs. Brown & Lenox. The press at Woolwich is also furnished with a weighted valve, according to the plan just mentioned, and in addition to the system of levers. The lever scale is perfectly sensible to a few pounds, but the valve scale will scarcely move with a load of two tons, and it is less and less sensible as the loads increase. The balanced levers are perfectly accurate, but the apparatus is rather expensive. At the last Worcester Show of the Royal Agricultural Society, a certain apparatus (not patented) was exhibited for testing the draft of Fowler's six-furrow steam plough, and it appears to me that a modification of this dynamometer might be employed for registering the stress on a cable. It consisted essentially of a cylinder, and a piston, on one side of which was a volume of water in communication with a Bourdon gauge. The water was enclosed in an elastic diaphragm, fixed to the piston and to the cover, and the gauge was necessarily marked according to the results given by weights gradually applied. By shrinking rings on the outside, or by straining on a coil of wire, the cylinder could be made to stand any amount of pressure required, and, if adjusted with the cross shackle pins at the opposite ends, at right angles to each other, in order to prevent any torsion, and also by the adoption of other simple means, such as the use of steel, that will occur to those now present, a light instrument of probably very great delicacy would be obtained.

When a long length of cable, say of seventy-five fathoms, is being tested, there is another influence that will, in some cases, affect the result. If we take the comparatively light one-inch cable, we find that it weighs 58 lbs. per fathom, so that the whole length will weigh nearly two tons. The last link at each end will have to stand a down pull of nearly one ton in addition to the longitudinal stress of 18 tons. This, however, would probably be practically compensated by directing the hammer test more towards the centre portions of the cable, and the *vis viva* of each blow will be absorbed by the elasticity of the metal, the deflection of each link struck, and by the combined weight and resilience of a certain portion of the cable within the range of each blow. It may here be noticed that in testing the effect of impact on beams, Mr. Hodgkinson used a 4 lb leaden cushion in order to partially deaden the jar of the blow. In a leading article of one of the engineering journals, in May last year, giving an account of Lloyd's proving-house, it was proposed that "a falling weight, to be released by a trigger tripped by a long cord," should be employed instead of the hammer, in order to prevent any accident to the operative, through the flying of the cable or a chip of the cast iron cross-stays. This weight could be made to slide overhead in the same vertical plane as the cable; and by letting it fall from the heights determined for each diameter of cable, the *vis viva* employed could be measured with approximate accuracy. This would only be on a par with the plan adopted in numberless instances, as we have seen, by our scientific neighbors the French; and similar measures might perhaps be used to measure the blow required to carry out the fracture test.

The application of known impulsive force as a test is of the utmost value, more especially when, as with cables, the object tested will have to undergo such forces in practice. If some plan could be devised for easily and accurately submitting the whole length simultaneously to a sudden instead of a static load, this would be of great importance. In the meantime, the hammer-blows are the tests for the resilience of the cable. In doubtful cases Professor Daniell's acid test might be of value in examining the structure of the fractured sections of the two or three links that are usually broken up. A great number of experiments on the specific gravity of iron have shown that it would be dangerous to make deductions as to the qualities of a specimen of wrought iron worked by one metallurgical process, and to then apply these results to a bar produced by another mode—for instance, to campare in this way a rolled bar with a hammered bar. At the same time there is a remarkably close, though not perfect, correspondence between the specific gravity and the quality of the specimens. Mr. Kirkaldy found that the specific gravity of iron was even decreased by being much strained—at any rate by tension. It is very easy to obtain the specific gravity of any substance like iron; and whether the physical facts that, 1st, the gravities of, for instance, No. 3 bars, bear a pretty constant relation to their qualities; and that, 2d, the specific gravity of wrought iron generally is diminished by tensional straining; and, 3d, that it is considerably increased by annealing, might be used in practice for testing the quality of the iron, or the deterioration through wear of a chain, is at least worth an inquiry.

The physical conditions involved in the construction, the use, and the testing of anchors, differ so materially from those of chain cables, that the two subjects must be separated in an examination of this kind. But there can be little doubt that a sound and general system of testing the mooring tackle of ships will bring about the same improvement in the quality of chain cables and anchors, as the trials at Shoeburyness have already effected in the quality of rolled plates; and the effect will indeed be produced by somewhat similar causes.

(To be Continued.)

For the Journal of the Franklin Institute.

Bars at the Entrance of Harbors on Tide-water. By D. S. Howard, C. & M. Eng.

The same general laws will apply to the formation of bars in tidewater, when formed by currents either at the mouths of rivers or by the tide at the entrance of bays, but most bars at the entrance of bays, when the bays are of a capacity to quiet the current of the streams entering them, or those receiving no streams, are formed by winds blowing parallel, or nearly so, with the coast, where material exists which may be removed by the waves, and still heavy enough to sink in the heaviest sea-water.

The conduct of water, under the influence of wind, is well known to the most casual observer, so far as it may be seen on the surface, but the under currents in the constitution of waves are not so well understood. In deep water, with a steady constant wind, we see the swells large and smooth, with no disposition to translate anything on their surface horizontally in any direction. These waves continue in unobstructed sea room, far beyond the direct influence of the wind; they then become what are called "dead swells," and continue in that character until they approach shoal water, when the under current becomes quickened by contraction, and the disposition of the wave is determined by the form of the bottom. If a gradual smooth shoal, until they reach the shore, the waves become shorter and more curved on the back, as the under current is more and more increased by contraction, until they break, forming a crest-like appearance, (they are then called breakers,) the surface water sometimes rolling back and the remainder rolling forward until they reach an elevation on the shore indicated by the perpendicular height and the momentum of the swell.

These breakers show more or less distinctly the different currents constituting waves, which are, undoubtedly, generated in the incipient sea, continued through the dead swell, and fully developed in the breaker, showing conclusively the formation of a vertical eddy under every wave. When the winds are parallel to the coast, these vertical eddies at certain depths of water, indicated by the force of the wind and the size of the wave, take up whatever loose material may be in their way, and deposit again in the direction of the motion of the wave, at a distance of one wave from the other, each one taking it up and depositing it as before, until it reaches a depression in the bottom like the entrance to a bay or harbor, where it may rest out of the reach of that depth of wave.

There is a difference between the vertical eddies under waves and those formed by a current rising from deep to shoal water, such as form bars in rivers, and sometimes at the entrance to harbors in tide-water; these are stationary as a whole, like their cause, while those partake of the same movements of their parent waves, and require a deeper place for a permanent deposit, while these require a shoaler place, and must also be furnished with material by an extended current, like that of a river, while those pick up their own material wherever the trough of the sea approaches the bottom sufficiently to create the necessary force to the under current.

There are few outer bars now formed in tide-water, not traceable to the action of waves, except such as that at the mouth of the Mississippi. There are also few rivers in this age of the world, with anything like a fixed regimen, emptying into tide-water, that have not extended their delta so far into the ocean as to diminish the force of their currents so much as to render them incapable of conveying any material of a size and density of substance sufficient to sink in sea-water.

The immense amount of water passing out at the mouths of the Mississippi river at the time of freshets, is such that the salt-water is forced out beyond the bar, allowing the deposits to be made in fresh water, by a vertical eddy formed by the lower current of the river rising from deep water a short distance above, where they remain as long as the freshet is sufficient to keep the heavier salt water from the bar. When the high water subsides sufficiently, the heavier salt-water of the Gulf finds its way, through the help of the wind and tide, into the river so far as to float away the bar, partly in the form of what are called "mud lumps," which arise to the surface in large bodies, and are driven ashore, or out to sea, as the stronger winds or tides may prevail, and partly by a semi-fluid, formed by the mingling of the salt-water with the material of the bar, both being so near the same weight as to render the deposits subject to similar laws of translation, consequently are gradually disposed of along the coast by the winds and waves, transferring shoals into islands and peninsulas, forming the bays, which abound on the Gulf coast.

It is well known that there is plenty of water over the bar at the mouth of the Mississippi except in high freshets, when the salt water is forced beyond the bar. Still, it is also well known, that there is no time, even in the lowest water, when there is not a large amount of material brought down from the turbulent Missouri, of the same character as that which comes in with the freshets, but no deposit is made on the bar; some may settle in deep fresh water above, to be swept out by the lower current of the freshets, and remain on the bar until floated away by the salt-water when the freshets subside; then the heaviest salt-water forces its way under the fresh far above the bar, in flood-tide, loosening up the deposits and preparing them for removal by the ebb-tide, assisted by the flow of the river, which determines their destination outwards into the Gulf.

It would seem, from the most reliable facts which may be gathered from experience and observation, that the only true way to prevent bars from forming at the entrance to harbors in tide-waters, is to construct piers on each side of the channel, near enough together to prevent the coasting winds from creating a sea from one to the other; then dredge or scour out the channel to a uniform depth, so that, if there should be any silt or sediment floating in or out of the harbor by the tide or river current, there would be no tendency to deposit by the action of vertical whirls or eddies created by elevations or depressions in the bottom.

This has been the most approved plan by the most eminent engineers, but the difficulty in many cases in obtaining stone for the construction of piers has led to the conclusion, that nearly all of our harbors south of New York, and many others on the Atlantic coast, are out of the pale of improvement; but, in my opinion, a few simple experiments, properly conducted, would prove that the prime ingredient of a concrete, abundantly sufficient for the purpose, can be found wherever bars are formed, and procured at a cost that would settle the question of expense in favor of the practical improvement of any harbor on the Atlantic coast.

Elwood, N. J., Sept. 1, 1864.

On the Machinery employed in sinking Artesian Wells on the Continent. By G. R. BURNELL, F.G.S.

It was stated that the extraordinary depth of some borings lately made for the purpose of obtaining an Artesian supply of water had led to great changes in the well-borers' art, and to the introduction of various mechanical processes, and of modifications of machinery, into the merits of which it was desirable to inquire.

The first well of this kind was that at the Abbatoir of Grenelle. This was sunk, after eight years incessant labor, a total depth of 1797½ ft. (English,) and gave rise to many inventions for the purpose of facilitating the progress of the works, for removing the broken tools, for the introduction of the pipes, and for carrying on observations at various depths from the surface. Subsequently many similar wells were sunk on the Continent, particularly in the Rhine provinces, but they were all of small diameter. The German engineers introduced important modifications in the tools. Æneyhausen made the striking part used for comminuting the rock to slide, so as to fall always through a certain distance, and thus avoid a jar. Kind had already applied his system to large excavations for winning coal mines, when he was entrusted by the Municipal Councils of Paris, with the execution of the well at Passy. This was to have a diameter of one metre (3 feet $3\frac{7}{16}$ in.), that of Grenelle being only 20 centimetres (about 8 inches). The difficulties encountered in carrying the excavation through the clays of the upper series were so serious that six years and nine months were occupied in reaching the water-bearing stratum, which was ultimately attained at a depth of 1913 ft. 10 ins. from the surface, when the yield was 3,349,200 gallons per day of twenty-four hours, subsequently increased to 5,582,000 gallons and then continued at 3,795,000 gallons per day. The total cost of the well was £40,000. It was lined with solid masonry for a depth of 150 feet; then wood and iron tubing was introduced to 1804 feet from the surface, and below that there was a length of copper pipe pierced with holes.

The results of this well had been so satisfactory, as regarded the quantity of water, that the authorities had decided to execute at once two additional Artesian wells, and there were rumors that two others were contemplated.

There were three different systems of well-boring, mostly dependent on the nature of the tools: the Chinese, or M. Fauvelle's; the French, or rather the usual well-borers' plan; and M. Kind's. In the first, the motion given to the tool in rotation was simply derived from the resistance that a rope would excercise to an effort of torsion, and therefore the limits of application of the system were only such as would allow the tool to be safely acted upon. Besides, a considerable quantity of water was required to clear out the boring, so that this plan had been almost universally abandoned. In the ordinary system of well-boring, the weight of the tools and of the solid iron rods became so great when the excavation was deep, that there was considerable difficulty in transmitting the blow of the tool, in consequence of the vibration produced in the long rod, or in consequence of the torsion. Hollow rods filled with cork, and M. Æneyhausen's joint, which permitted the tool to fall freely and through the same height every time it was released, were now employed. M. Kind adopted both these modifications, and in the well of Passy he substituted oak rods for iron ones, as being lighter and more easily counterbalanced in water. The products of the excavation were still most frequently removed by augers and chisels; and all the processes hitherto practised were considered to be more or less defective, as in every case the comminuting tool had to be withdrawn. In the well at Passy, M. Kind employed a trepan to comminute the rock; it weighed 1 ton 16 cwt. and fell through 2 feet. This tool was composed of two principal pieces—the frame and the arms—both of wrought iron, but the teeth of the cutting part were of cast steel. The frame had at the bottom a series of holes slightly conical, into which the teeth were inserted, and were tightly wedged up. These teeth were placed with their cutting edge on the longitudinal axis of the frame that received them; and at the extremity of the latter there were formed two heads forged out of the same piece with the body of the tool, which also carried two teeth placed in the same direction as the others, but which were made of double the width of the latter in order to render this part of the tool more powerful. It was by increasing the dimensions of these end-teeth that the diameter of the boring could be augmented, so as to compensate for the diminution of the clear space by the tubing that it might be necessary to introduce in traversing strata disposed to fall in, or to allow the waters from below to escape at an intermediate level. Above the lower part of the frame of the trepan was a second piece, composed of two parts bolted together, and made to support the lower portion of the frame. This part of the machinery also carried two teeth at its extremities, which served to guide the tool in its descent, and to work off the asperities that might be left by the lower portion of the trepan. Above this again were the guides of the machinery properly speaking, consisting of two pieces of wrought iron arranged in the form of a cross, with the ends turned up, so as to preserve the machinery perfectly vertical in its movements, by pressing against the sides of the boring already executed. These pieces were independent of the blades of the trepan, and might be moved closer to it or further away from it as might be desired. The stem and arms were, lastly, terminated by a single piece of wrought iron, which was joined to the frame by a kind of saddle joint, and was kept in its place by means of keys and wedges. The whole of the trepan was finally jointed to the great rods, that communicated the motion from the surface, by means of a screwed coupling, formed below the part of the tool that bore the joint which permitted the free fall of the cutting part, and united the top of the arms and frame and the rod.

Proc. Inst. Civil Eng., May 24, 1864.

The Adhesion of Krupp's Patent Steel Tyres.

From the London Mechanics' Magazine, June, 1864.

From a recent return made by the machinery department of the Chicago, Burlington & Quincy railway, we glean the following interesting facts respecting the traction of Krupp's Patent Steel Tyres. Inasmuch as some locomotive superintendents have had their doubts as to the adhesion of this tyre, the data contained in this report becomes the more important and valuable:

It appears that a set of Krupp's Tyres have been running under engine "34" thirteen months. The weight of the engine is 34 tons, and it is used in the heaviest goods service. During ten days of the past winter, this engine was behind a snow-plough with three engines back of it. It was mainly relied on for backing out of a snow-bank, and its wheels were found to slip less and to bite stronger than those tyred with iron. The engine was watched very closely through this severe test, which is regarded by the company as conclusive of the perfect traction of the tyre.

The wear in thirteen months is very slight, and the tyre is considered good for another year before going in for the first turning. The tyre retains its perfect circularity.

The toughness of the Krupp steel affords an easy solution of this traction problem, which has caused much speculation in the minds of many railway managers.

For the Journal of the Franklin Institute.

General Problem of Trussed Girders. By De Volson Wood, Prof. of C. E., University of Michigan.

I wish in the following article to show how to investigate truss bridges from the fundamental equations of statics.

The Problem. *It is proposed to find the strains upon the several pieces which form a truss when acted upon by any system of forces.*

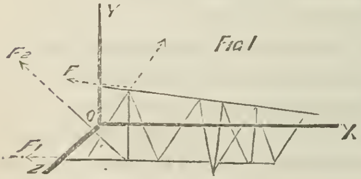
The demonstration is founded upon the following proposition, which follows directly from statics.

Proposition. *If a frame of any form or combination be acted upon by any system of external forces, and the frame be completely divided into two parts by an ideal plane, the strains which act along the bars which are divided by the plane, are in equilibrium with the external forces which act upon the part of the frame on either side of the intersecting surface.*

We may readily conceive that the forces on one side of the ideal plane, hold the frame for the others to act, and the pieces transmit the forces (strains) from one part to the other; hence we may conceive that the forces on *either* side of the plane cause the strains.

We have then only to substitute forces for the strains and treat of them according to the principles of statics.

Let fig. 1 represent the combination of bars (or pieces). Take the origin of co-ordinates at O anywhere in the plane of section.



Let Y, be vertical and positive upwards.
X, horizontal and positive on the side of the section that the forces are considered.
Z, also horizontal and positive to the right of X as we look from Y towards O.

The precautions in regard to the signs are only necessary to enable us to use the same sign throughout the discussion and the applications.

Let P, P_1, P_2, &c., be the applied forces,
a, b, and c, the angles which P make with X, Y, and Z, respectively; and the same letters with subscripts, the corresponding angles of P_1, P_2, &c.
F, F_1, F_2, &c., the strains on the bars which are intersected by the plane.
α, β, and, γ the angles of F with X, Y, and Z, respectively, and the same letters with subscripts for the corresponding angles of F_1, F_2, &c.,

ΣX the sum of all the X components of the forces; similarly for ΣY and ΣZ.

Now conceive that the frame is divided and one part removed, and that in the plane section forces are substituted which will cause the same strains on the intersected bars that existed before the section was made; it is evident that the forces are in equilibrium the same as before one part was removed.

As the forces are in equilibrium we have

$$\begin{aligned}
&\text{the forces:} && \Sigma X = 0\\
& && \Sigma Y = 0\\
& && \Sigma Z = 0\\
&\text{and the couples:} && \Sigma Zy - \Sigma Yz = 0\\
& && \Sigma Xz - \Sigma Zx = 0\\
& && \Sigma Yx - \Sigma Xy = 0
\end{aligned}$$

which by development gives—

$$\text{the forces: }\left.\begin{aligned}
&P\cos a + P_1\cos a_1 + \&c. + F\cos\alpha + F_1\cos\alpha_1 + \&c. = 0\\
&P\cos b + P_1\cos b_1 + \&c. + F\cos\beta + F_1\cos\beta_1 + \&c. = 0\\
&P\cos c + P_1\cos c_1 + \&c. + F\cos\gamma + F_1\cos\gamma_1 + \&c. = 0
\end{aligned}\right\} \quad (1)$$

and the couples

$$\left.\begin{aligned}
&\text{around } X, \Sigma P\,(y\cos c - z\cos b) + \Sigma F\,(y\cos\gamma - z\cos\beta) = 0\\
&\text{“}\quad Y, \Sigma P\,(z\cos a - x\cos c) + \Sigma F\,(z\cos\alpha - x\cos\gamma) = 0\\
&\text{“}\quad Z, \Sigma P\,(x\cos b - y\cos a) + \Sigma F\,(x\cos\beta - y\cos\alpha) = 0
\end{aligned}\right\} \quad . \quad (2)$$

Equation (1) may be written more concisely, thus:

$$\left.\begin{aligned}
\Sigma P\cos a + \Sigma F\cos\alpha &= 0\\
\Sigma P\cos b + \Sigma F\cos\beta &= 0\\
\Sigma P\cos c + \Sigma F\cos\gamma &= 0
\end{aligned}\right\} \quad . \quad . \quad . \quad (3)$$

Discussion of equations (1) *and* (2).

1°. In the earlier part of the discussion we will suppose that the applied forces, P, P_1, &c., are given, and hence their components may be readily computed. Considering them as known quantities and placing them in the second member we have—

$$\text{the forces: }\left.\begin{aligned}
\Sigma F\cos\alpha &= -\Sigma P\cos a\\
\Sigma F\cos\beta &= -\Sigma P\cos b\\
\Sigma F\cos\gamma &= -\Sigma P\cos c
\end{aligned}\right\} \quad . \quad . \quad . \quad (4)$$

and the couples;

$$\left.\begin{aligned}
&\text{around } X, \Sigma F\,(y\cos\gamma - z\cos\beta) = -\Sigma P\,(y\cos c - z\cos b)\\
&\text{“}\quad Y, \Sigma F\,(z\cos\alpha - x\cos\gamma) = -\Sigma P\,(z\cos a - x\cos c)\\
&\text{“}\quad Z, \Sigma F\,(x\cos\beta - y\cos\alpha) = -\Sigma P\,(x\cos b - y\cos a)
\end{aligned}\right\} \quad . \quad (5)$$

2°. In (4) and (5) there are six independent equations; hence the problem is determinate, and may be solved by elimination, if the plane intersects six bars or less. If it intersects more than six it is indeterminate, unless conditions are established among the strains.

3°. If the frame has an axis, let X coincide with or be parallel to it, then we call

$\Sigma P \cos a$ the DIRECT STRESS, $\Sigma P \cos b$ and $\Sigma P \cos c$ the total SHEARING STRESS, $\Sigma P(\cos b - y \cos a)$ the BENDING COUPLE, and the other two the TWISTING COUPLES.

Practical examples of such a general character very rarely occur.

4°. The strains upon the bars depend upon their position, and not upon the trussing between them and either end.

5°. Let all the forces be in the plane XY. Then will $c = 90°$, $\gamma = 90°$, $(a + b) = 90°$, and $(\alpha + \beta) = 90°$, and equations (4) and (5) become

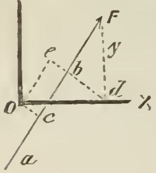
$$\left.\begin{aligned} \Sigma F \cos \alpha &= -\Sigma P \cos a \\ \Sigma F \cos \beta &= -\Sigma P \cos b \text{ also} = \Sigma F \sin \beta \\ \Sigma F(x \cos \beta - y \cos \alpha) &= -\Sigma P(x \cos b - y \cos a) \end{aligned}\right\} \quad (6)$$

in which the first is the *direct stress*, the second the *shearing stress*, and the third the *bending couple*.

The quantity $(x \cos \beta - y \cos \alpha)$ is the length of the perpendicular let fall from the origin on F. For, let ab, fig. 2, be a bar along which F acts; O the origin.

The point of application of the force may be anywhere along ab; let it be at F, and draw Fd perpendicular to X; db and Oc perpendicular to ab, and Oe parallel to ab.

FIG 2



Then $Od = x$; $dF = y$, $bdF = \alpha$, and $bFd = \beta$

$bd = y \cos \alpha$, $ed = x \cos \beta$

$\therefore Oc = eb = ed - bd = x \cos \beta - y \cos \alpha$.

Call this perpendicular q, and the second member Mz, and the equation becomes $\Sigma Fq = -Mz$.

6°. From (6) we see that the problem is determinate when the plane section intersects three bars. If more, conditions must be established among the strains.

Suppose the plane intersects three pieces; viz: the upper and lower chords and a brace (or tie), and

let F = the strain on the upper chord;
F_1 = the strain on the lower chord;
F_2 = the strain on the brace;
α, α_1 and α_2, the corresponding angles with X,
q, q_1 and q_2, the lever arms of the strains.

Then (6) will become

$$\left.\begin{aligned} F \cos \alpha + F_1 \cos \alpha_1 + F_2 \cos \alpha_2 &= -\Sigma P \cos a \\ F \sin \alpha + F_1 \sin \alpha_1 + F_2 \sin \alpha_2 &= -\Sigma P \cos b \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 &= -\Sigma P(x \cos b - y \cos a) \end{aligned}\right\} \quad (7)$$

EXAMPLE. Suppose that while the frame ABC, fig. 3, is being put in position, it is acted upon by the three forces P, P_1, P_2.

Let the frame be 50 feet long and weigh 100 lbs. per foot.

$$\text{Also } P_1=200 \text{ lbs.};\ \beta_1=150^\circ \quad \therefore \quad a_1=-60^\circ$$

$$P_2=300 \text{ lbs.};\ \beta_2=170^\circ \quad \therefore \quad a_2=-80^\circ$$

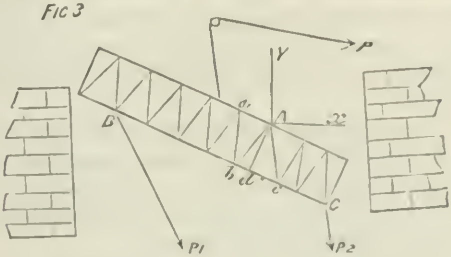
from which we may find P and a; for we have

$$\Sigma X = P\cos a + 200\cos 60^\circ + 300\cos 80^\circ = 0$$

$$\Sigma Y = P\sin a + 200\sin(-60^\circ) + 300\sin(-80^\circ) - 50\times 100 = 0$$

$$\therefore \quad P = 5470 \text{ lbs};\ a = -88^\circ\, 24\tfrac{1}{2}.$$

FIG 3



Now suppose we wish the strains on Aa, Ab, and bc. Intersect these bars by a vertical line or plane, and to simplify the case take the intersection infinitely near A. Let Aa and bc be parallel, and make an angle of —30° with X, and Ab an angle of +20°; Ad = q = 6 feet, q and q_2 are zero.

According to the proposition, we may consider the forces on the right or left of A, and as there is but one external force on the right we will consider the forces on that side. Suppose there is 20 feet of frame on the right of A, and the center of gravity is 10 feet from Y; then will the moment be $20\times 100\times 10 = 20000$. Call the lever arm of P_2 15 feet. The strain on Aa is tensive, hence $a = 150^\circ$; on bc it is compression, hence $a_1 = -30^\circ$; on Ab it is compression, hence $a_2 = 20^\circ$. Hence equation (7) become

$$F\cos 150^\circ + F_1\cos(-30^\circ) + F_2\cos 20^\circ = -\ 152{\cdot}08$$

$$F\sin 150^\circ - F_1\sin\ 30^\circ + F_2\sin 20^\circ = \quad 5468{\cdot}64$$

$$-F_1\times 6 \quad = -(20000+300\times 15)$$

The signs of 6 F_1 is minus because it tends to turn the system from right to left—the signs of the other moments are positive because they tend to turn it in the opposite direction. From these we find

$$F = 10862 \text{ lbs},\ F_1 = 4083\tfrac{1}{3} \text{ lbs. and } F_2 = 6082 \text{ lbs.}$$

7°. Let all the forces be vertical: or, $b = 0^\circ$ or 180,° $a = \pm 90^\circ$ These in (7) give:

$$\left.\begin{aligned} F\cos a + F_1\cos a_1 + F_2\cos a_2 &= 0 \\ F\sin a + F_1\sin a_1 + F_2\sin a_2 &= -\ \Sigma \pm P \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 \quad &= -\ \Sigma \pm Px \end{aligned}\right\} \qquad (8)$$

The plus sign belongs to forces which act vertically upward. It is well here to observe that if we consider tension as positive we *may* consider compression as negative; or we may consider both as positive and attribute the sign to the trigonometrical function—the same that we do with the applied forces—in which case a compression may be considered as a thrust whose direction is changed 180.° The hypotheses are essentially the same, although the latter is the one commonly used in establishing equations.

The first of equations (8) shows that the total compressive strain equals the total tensile ones.

The *shearing* stress, as found by the second of equations (7), *is greater on any vertical section when the longer part of the frame into which it is divided by the plane, is loaded and the shorter part unloaded.*

For if we conceive the longer part loaded—say from V, to the section, in fig. 5, and the remaining part unloaded—then will the total shearing stress on all vertical sections between the plane section and the other end be the same, and equal the reaction of the support, V; but if any load be now added to the shorter part, V will sustain a part of it while all of it must be subtracted from V to find the *shearing* stress on the section; hence it is less than before any load was placed on the shorter part.

It is easy to show that the sum of the moments, ΣPx, as given by the third of (7) is greatest when the frame is loaded throughout.

Equations (8) are applicable to trusses of all forms, but they may be reduced so as to be more convenient for particular cases.

8°. Let both chords be convex upwards.

If the space occupied by the truss be divided into trapezoids by vertical bars, as *ac* and *bd*, fig. 5, and the diagonal ones so arranged that only one shall act at a time—or what is equivalent, the diagonal bars shall be so arranged as to be subject to only one kind of strain, I shall call it the *panel* system. But if it be divided into triangles, as in fig. 4, I shall call it the *triangular* system.

FIG 4

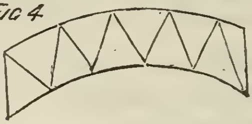
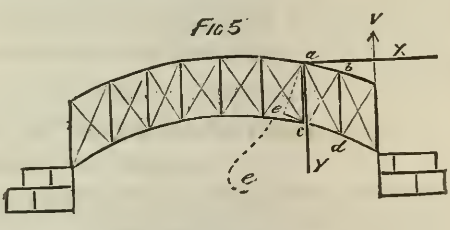


FIG 5



A bar makes the same angle with the axes that a line does which is drawn through the origin and parallel to the bar. We may assume any position of the bar to make positive angles with the axes, but after assuming the position we observe that a bar which inclines the opposite way makes a negative angle—but to be more specific we will assume, that when a line, or its prolongation, which is drawn through the origin and parallel to the bar, falls between $+X$ and $+Y$ the angle is positive—otherwise, it is negative. Still further, to avoid numerous negative angles which would otherwise occur in the position which we shall assume for the origin, I will make Y positive downwards. This will be equivalent to changing the signs of the vertical forces, and equations (8) become

$$\left.\begin{array}{l} F\cos\alpha + F_1\cos\alpha_1 + F_2\cos\alpha_2 = 0 \\ F\sin\alpha + F_1\sin\alpha_1 + F_2\sin\alpha_2 = \Sigma \pm P \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 \quad = -\Sigma \pm Px \end{array}\right\} \quad . \qquad (9)$$

in which plus P acts vertically upwards.

To determine the angle of the strains we conceive that forces are applied to the bars in the section which shall produce the same strains as before the section was made; then draw a line from the origin parallel to, and in, the direction of the action of the force, and it will make the same angle with the axes that the force does. We also observe that $+\alpha$ is reckoned from $+X$ towards $+Y$.

To prepare equations (9) for use, we will make a section at the right of, but indefinitely near to a, in fig. 5.

Let i = the angle which the upper chord makes with X.
i_1 = the angle which the lower chord makes with X.
θ = the angle which the brace $a\,d$ makes with Y.
V = the vertical reaction of the support.
x_1 = the lever arm of V.
$h = ac$ = the vertical depth of the frame at the origin.
$\Sigma_0^x P$ = the sum of all the loads between the origin and the end.
$\Sigma_0^x Px$ = the moments of all the loads between the origin and V.

Take the origin at a. The strain on ab is compressive, to produce which requires that F act from a towards b; hence $\alpha = i$. F_1 must act at c, and towards c; hence $\alpha_1 = 180° + i_1$; and F_2 acts from a towards d; hence $\alpha_2 = 90 - \theta$. $q = 0$; $q_1 = ae = h \cos i_1$; $q_2 = 0$. These values reduce (9) to

$$\left.\begin{aligned} F\cos i - F_1\cos i_1 + F_2\sin\theta &= 0 \\ F\sin i - F_1\sin i_1 + F_2\cos\theta &= V - \Sigma_0^{x_1}P \\ F_1\, h\cos i_1 \qquad\qquad &= Vx_1 - \Sigma_0\, Px \end{aligned}\right\} \quad . \quad . \quad (10)$$

For maximum *shearing* stress $\Sigma_0^x P = 0$; and we have

$$\left.\begin{aligned} F\cos i - F_1\cos i_1 + F_2\sin\theta &= 0 \\ F\sin i - F_1\sin i_1 + F_2\cos\vartheta &= V \\ F_1 h\cos i_1 \qquad\qquad &= Vx_1 \end{aligned}\right\} \quad . \quad . \quad . \quad (11)$$

It must be observed that V in this case is not the same as in the preceding, but it must be computed from the given data.

Example. Suppose that in a bridge which is 120 feet long it is observed that at 30 feet from the end the upper chord is inclined 15° to the horizon: the lower chord 20,° the brace 45,° and the depth is 10 feet. It is required to find the strains upon these bars when there is an uniform load of one ton per foot over the whole length of the bridge: also the strains at the instant of maximum shearing, when it moves off without shock.

For the former part of the problem we use (10) and readily have

$$\left.\begin{aligned} F\cos 15° - F_1\cos 20° + F_2\sin 45° &= 0 \\ F\sin 15° - F_1\sin 20° + F_2\cos 45° &= 60 - 30 = 30 \\ 10\, F_1\cos 20° &= 60.30 - 30.15 = 1350. \end{aligned}\right\}$$

These solved give

$$F = 77,\ F_1 = 143, \text{ and } F_2 = 85 \text{ tons.}$$

The results are given to the nearest whole number.

For the maximum shearing, the load, according to the principles before stated, must extend over 90 feet. For this we find $V = \frac{90 \cdot 45}{120} = 33\tfrac{3}{4}$, which in (11) gives

$$\left.\begin{array}{r} F\cos 15^\circ - F_1\cos 20^\circ - F_2\sin 45^\circ = 0 \\ F\sin 15^\circ - F_1\sin 20^\circ + F_2\cos 45^\circ = 33\tfrac{3}{4} \\ 10F_1\cos 20^\circ \qquad\qquad = 1012\tfrac{1}{2} \end{array}\right\}$$

which being solved will give

$$F = 43{\cdot}2,\ F_1 = 107{\cdot}6,\ F_2 = 84{\cdot}4 \text{ tons},$$

which compared with the preceding shows that the strain on the brace is nearly as great as when the whole bridge is loaded. We shall hereafter see that there are cases in which the strain on the braces is *greater* for maximum shearing than when the whole frame is loaded.

If *bc* is brought into action as a brace, it must be done by the loading between *b* and V.

(To be Continued.)

For the Journal of the Franklin Institute.

The Cinematics of the Slide Valve. By ALBERT ASTON, U.S. Corps of Naval Engineers.

As experience has proved the slide valve, cutting off by lap, to be, of all devices, the best adapted to locomotive and screw-propeller engines, a set of reliable formulas, embracing all the practical conditions, for determining the proportions necessary for its proper action, will doubtless be acceptable to those engaged in its construction.

In the following discussion, strict mathematical accuracy is not aimed at, as that would only uselessly complicate the problem. The results given by the formulas, however, will, within the limits of practice, differ from the absolute truth only by insensible quantities.

The principal dimensions to be determined are the throw of the eccentric and the lap of the valve. For these we have given the length of stroke of the piston, or the length of the crank; the point of cutting off, and the width of opening of the steam-port.

The action of an eccentric is to be regarded as precisely the same as that of a crank, the centre of the eccentric corresponding to the centre of the pin of the crank. The distance from the centre of the shaft to the centre of the eccentric will be called, for convenience, the radius of the eccentric.

If a valve be without lead or lap, and the connecting-rod and eccentric-rod are supposed to be infinitely long, it will be in mid-position, or at half its stroke, when the piston is at the beginning of its stroke, its edges will coincide with the edges of the steam-ports, and its stroke will be exactly double the width of the port; but if lap on the steam side be given to the valve, its length will be correspondingly increased beyond the edges of the ports, consequently its stroke must be increased by exactly the increased length of the valve, and the centre of the eccentric must be revolved about the shaft from its mid-position until the extreme edge of the valve again coincides with the edge of the port, as the valve must be on the point of opening when the piston is at the commencement of its stroke.

It will be seen by inspecting the diagram, Plate III, that the valve will open while the centre of the eccentric passes through the arc *pd*, and that

Fig. 1.

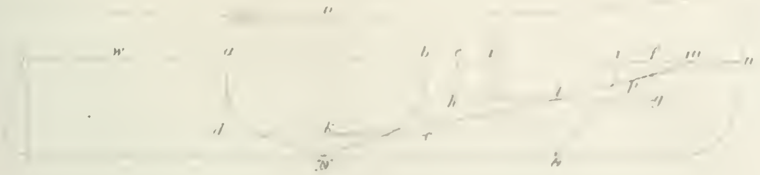


Fig. 3.

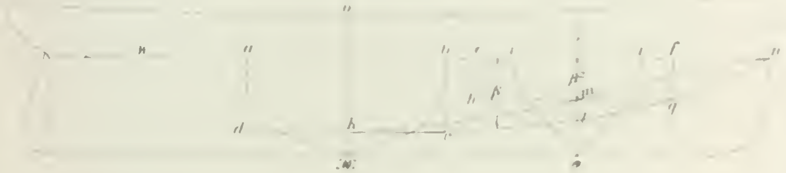


Fig. 2.

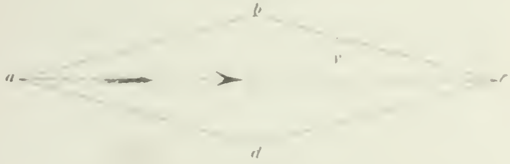


Fig. 4.

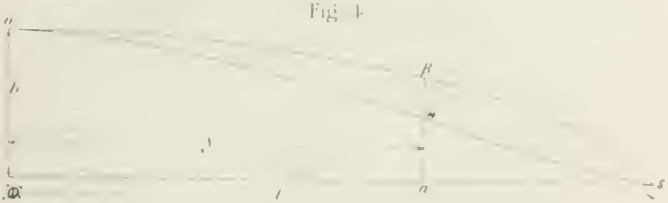
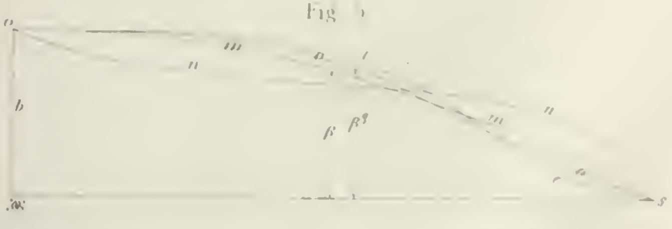


Fig. 5.



it will close while the same point passes through the equal arc dp', the points p and p' being equally distant from any point in the straight line AB; therefore, the versed-sine of the arc pd is equal to the width of opening of the steam-port. As the centre of the eccentric was revolved about the shaft through the arc mp on account of the lap, the distance through which the valve moved (which is equal to the lap) is equal to the sine of the arc mp, or, to the cosine of its complement, pd. If the centre of the eccentric should pass to the point p'', which is directly opposite the point p, the other end of the valve would be the point of opening.

Both the crank and eccentric being secured to the same shaft, it is evident they must pass through equal angles during any portion of a revolution. Thus, if the crank-pin pass from P to P′, the centre of the eccentric will pass from p to p', the angle PcP′ being equal to the angle pcp'. Therefore, if the valve be required to open and close while the crank-pin passes from P to P′, the centre of the eccentric must be secured at the point p when the crank-pin is at P, and the angle pcd equal to one-half of pcp', or one-half of PcP′.

To find the radius of the eccentric when the width of the steam-port and the angle PcP′ are given,

Let a = width of steam-port opening,
r = radius of eccentric,
β = angle PcP′ through which the crank has passed.

Then

$$a = r - r\cos\frac{\beta}{2}$$

$$a = r\left(1 - \cos\frac{\beta}{2}\right)$$

$$r = \frac{a}{1 - \cos\frac{\beta}{2}} \quad . \quad . \quad . \quad (1)$$

To find the value of $\cos\frac{\beta}{2}$ in terms of the stroke of piston and point of cutting off, let R = the length of the crank, L = the length of stroke to be accomplished with the full pressure of steam,

then

$$R\cos\beta = R - L.$$

$$\cos\beta = \frac{R - L}{R}$$

The formula for the cosine of half an arc is

$$\cos\frac{\beta}{2} = \sqrt{\frac{1 + \cos\beta}{2}}$$

$$\therefore \cos\frac{\beta}{2} = \sqrt{\frac{1 + \frac{R - L}{R}}{2}} = \sqrt{\frac{2R - L}{2R}}$$

Substituting this value in (1), we have

$$r - \frac{a}{1 - \sqrt{\frac{2\,\mathrm{R} - \mathrm{L}}{2\,\mathrm{R}}}} \quad . \quad . \qquad (2)$$

We have seen before, that the lap of the valve is equal to $r \cos \frac{\beta}{2}$ or, which is the same thing, $r - a$; therefore, if we represent it by x, we have

$$x = r - a \quad . \quad . \quad . \qquad (3)$$

These values for the radius of the eccentric and the lap of the valve are true, provided the valve has no lead, and supposing the eccentric-rod to be always parallel to a line bisecting the angle pcp'. As the former is seldom, and the latter never the fact in practice, it will be necessary to investigate the effect of the derangement thus caused. It will be seen, however, that these two causes of derangement act in opposition to each other, the one tending to increase, and the other to diminish the radius of the eccentric; and, with the amount of lead usually employed, if the eccentric-rod has much angularity, they become nearly equal, thereby neutralizing each other. We will first consider the lead.

If the lead should be given by moving the eccentric around on the shaft to any point, q, the valve would still close when the centre of the eccentric arrived at the point p'; or, the steam would be cut off when the crank had passed through an angle equal to $q\,c\,p'$, which is less than the angle $p\,c\,p'$, or its equal $\mathrm{P}c\mathrm{P}'$, thus bringing the point of cutting off nearer to the commencement of the stroke of the piston. On the other hand, if the lead should be taken from the valve, the valve would not close until the centre of the eccentric had passed the point p' to some point, q', which would cause the crank to pass through an angle equal to $p\,c\,q'$, which is greater than $p\,c\,p'$, thereby bringing the point of cutting off nearer the *end* of the stroke of the piston.

As the lead is usually but a small portion of the stroke of the valve, it will be sufficiently accurate, for practical purposes, to consider the angle $p\,c\,q$ equal to the angle $p'c\,q'$, and to take one-half the lead from the lap of the valve and give the other half by moving the eccentric around the shaft to a point, u, midway between p and q. This can be done whatever may be the position of the point p.

As the valve will now be partly open when the centre of the eccentric is at the point p, or p', it is evident that the radius of the eccentric may be lessened so that the versed sine of the arcs $p\,c\,d$ or $d\,c\,p'$ plus the distance the valve is open, may be equal to the width of the steam opening, a. The distance the valve is open being one-half the lead, if we represent the lead by l, we have for the value of the versed sine of the arc $p\,d$,

$$a - \frac{l}{2}$$

The other cause of derangement, the obliquity of the eccentric-rod, causes the valve to open wider at one end than at the other, thus: Instead of the width of opening of the steam-port being equal to the versed sine of the arc $p\,d$, or to a, it will be equal to $s\,d$, or, a minus the versed sine of the arc $p\,s$, while at the other end of the valve it will be equal to $s'd'$. As the width of steam opening assumed is supposed to be as small as it properly can be, the value of r should be increased so that the distance $s\,d$ will give the proper opening. To this end, it will be necessary to add the versed sine of the arc $p\,s$ to that portion of r represented by a. Let this versed sine be represented by v, and we will then have for the value of the versed sine of the arc $p\,d$.

$$a - \frac{l}{2} + v.$$

which may be substituted for a in (2), when the formula will become

$$r = \frac{a - \frac{l}{2} + v.}{1 - \sqrt{\frac{2\text{R} - \text{L}}{2\text{R}}}} \quad . \quad . \quad . \quad (4)$$

As v is a variable function of r, its value cannot be obtained in terms independent of that quantity; nor, is it a matter of sufficient consequence to require a rigorous mathematical determination. If the value of r be first found by equation (2), and v be computed from that value, the result given by (4) will be found sufficiently accurate. The length of eccentric-rods is seldom less than fifteen times r; and it is easily seen that these inaccuracies are practically imperceptible. If v is found to be nearly equal to $\frac{l}{2}$ both may be neglected.

To find the value of v, let r' equal the length of the eccentric-rod, and ϕ equal the angle formed by the rod with the line AB, then

$$v = r' - r' \cos \phi.$$

$$r' \cos \phi = \sqrt{r'^2 - \left(r \sin \frac{\beta}{2}\right)^2},$$

$$\therefore\ v = r' - \sqrt{r'^2 - \left(r \sin \frac{\beta}{2}\right)^2},$$

but

$$r \sin \frac{\beta}{2} = r\sqrt{\frac{1 - \cos \beta}{2}} = r\sqrt{\frac{1 - \frac{\text{R} - \text{L}}{\text{R}}}{2}} = r\sqrt{\frac{\text{L}}{2\text{R}}},\ \text{and}$$

$$\left(r \sin \frac{\beta}{2}\right)^2 = \frac{\text{L}r^2}{2\text{R}}$$

$$\therefore\ v = r' - \sqrt{r'^2 - \frac{\text{L}r^2}{2\text{R}}} \quad . \quad . \quad (5)$$

The equation for the lap will now become

$$x = r\cos\frac{\beta}{2} - \frac{l}{2} = r\sqrt{\frac{2\text{R}-\text{L}}{2\text{R}}} - \frac{l}{2} \quad . \qquad (6)$$

which will be true whether $\frac{l}{2}$ and v are discarded from (4) or not.

The effect produced by the obliquity of the connecting-rod, is that of causing the valve to close sooner at one end than at the other, or, the steam will be cut off shorter at one end of the cylinder than at the other. The greater the obliquity of the rod, the greater will be the difference; but it may be partially remedied by giving the valve more lead at one end than at the other.

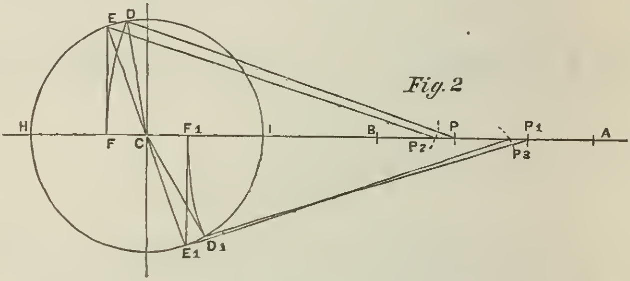


Fig. 2

In fig. 2, let AB represent the length of stroke of the piston or cross-head, and PP_1, points of cutting off equally distant from the ends A and B; then, taking the length of the connecting-rod, BH, as a radius, and the points P and P_1, as centres, describe the arcs FD and F_1D_1, which will cut the circumference described by the centre of the crank-pin in the points D and D_1. These points will represent the position of the crank-pin at the moment the steam is cut off; and it will be observed that the arc ID through which the pin passes on its outward stroke, is less than the arc HD_1 traversed by the pin on its inward stroke. Now, if the connecting-rod be supposed infinitely long, it will be always parallel to the line AH; the lines FD and F_1D_1, instead of being arcs, would be parallel straight lines; and, as they are equidistant from the centre of the circle described by the crank-pin and perpendicular to the line IH, they would be equal and would intercept equal arcs, IE and HE_1, of the circumference. If the points E and E_1 are assumed for the positions of the crank-pin at the time the steam is cut off, the cross-head must travel a distance greater than AP on its outward stroke, and a distance less than BP_1 on its inward stroke, for the arc IE is greater than the arc ID, and the arc HE_1 is less than the arc HD_1. Now, if we take the length of the connecting-rod, PF, as a radius, and the points E and E_1 as centres, and describe small arcs cutting the line AB in P_2 and P_3, these points will represent the positions of the cross-head, and the distances PP_2 and P_1P_3 will be equal; for the right-angled triangles FP_2E and $\text{F}_1\text{P}_3\text{E}_1$ are equal, and the difference between two adjacent sides of one, is equal to the difference between two corresponding adjacent sides of the other. As AP_2 is equal to $AP+PP_2$, and BP_3 is equal to $BP_1-P_1P_3$, the distance AP or BP_1 is an arithmetical mean between the distances AP_2 and BP_3.

To find the distance PP_2 or P_1P_3, we have

$$PP_2=P_2E-\sqrt{P_2E^2-EF^2}.$$

The quantity $EF^2=(R\sin\alpha)^2=R^2-(R\cos\alpha)^2=R^2-(R-L)^2$.

Let the distance PP_2 be represented by s, and the length of the connecting-rod by C, then

$$s=C-\sqrt{C^2-[R^2-(R-L)^2]}, \quad . \quad . \qquad (7)$$

which must be added to the mean distance of cutting off on the outward stroke and subtracted from it on the inward stroke.

If levers intervene between the eccentric and valve so that the stroke of the latter is increased or diminished, its dimensions must be regulated proportionately to the arms of the levers. This, however, will not apply to the Stephenson link, as the backing eccentric deranges the motion unless the axis of the forward eccentric-rod pin coincides with the axis of the link-pin. The usual method of attaching eccentric-rods to one side of the link, causes a slight derangement, but it is barely perceptible. The best way of finding the position of the link for intermediate points of cutting off, is as follows:—When the engines are set up and the valve-gear adjusted (the valve-chest cover being off if practicable), turn the shaft until the cross-head arrives at the point desired. Then move the link until the steam-port is just closed, and mark the position of the catch, on the guard or whatever other device may be adopted. Next, turn the shaft until the cross-head is in a corresponding position on the return stroke; move the link until the port is closed and mark the position of the catch. Cut the notch midway between the marks on the guard and proceed in the same manner for the other points of cutting off.

The only remaining question is that of the exhaust lap. If the exhaust lap was equal to the steam lap, the exhaust port would be closed at the same time as the steam-port, which would cause excessive cushioning. If, on the contrary, there was no exhaust lap, the exhaust-port would be open long before the steam-port, and, consequently, before the piston had arrived at the end of its stroke. The loss due to this too early release of the steam is more serious than that due to cushioning, for it is all a loss of power; whereas the compressed vapor partially or wholly fills the port and clearance which would, otherwise, have to be supplied with fresh steam from the boiler. In fact, if the expansion were carried down to the back-pressure, there would be no loss of economic effect by the cushioning, however excessive. The best relative proportion which these two losses should bear to each other, is, evidently, that in which the sum of the two would be a minimum. This could be easily determined by means of the differential calculus if the curve traced on the indicator card by the escaping steam, and which is dependent on the proportion between the valve-opening and the cylinder capacity, and the speed of the piston between the point of release and the end of its stroke, could be known, and if the compressed vapor followed Mariotte's or any known law; but as the exhausting curve cannot be easily determined, and as the required condition is never fulfilled by the cushioning steam, accurate calculation is out of the question. It should be remembered, however, that the valve should open long enough before the end of the stroke to allow the piston to commence its return stroke with the maximum vacuum, and that the cushioning is not perceptible on the indicator card until some time after the exhaust-port is actually closed, owing to the rapid condensation of the compressed vapor; and were it not for the atmospheric air mixed with the steam in the cylinder, the cushion curve would be much less than it is actually found to be, and a much earlier closing of the exhaust would be practicable. The problem is also affected by the absolute amount of back pressure; but it is found, from the inspection of a large number of indicator cards, that the most satisfactory diagrams are obtained when the exhaust lap is about one-half the steam lap.

In order to find the distance of the piston from the end of the stroke at the time the communication with the condenser is cut off, it will be necessary to find the angle previously passed through by the crank or the centre of eccentric. This angle is composed of the angle, *ucu'*, through which the centre of the eccentric passed before the steam was cut off, and the angle, *u'cw*, due to the difference between the steam and exhaust lap. As these angles are measured by their cosines on the line AB, we must suppose the angle *ucw* to be made up of the angles *ucd* and *dcw*. Let θ=the angle *ucw*; x'=the exhaust lap, and $c=\mathrm{R}-\mathrm{R}\cos\theta$=the distance of the piston from the end of the stroke,

then $$\cos\theta=\cos\left[\cos^{-1}\left(r\cos\frac{\beta}{2}-\frac{l}{2}-(x-x')\right)+\cos^{-1}\left(r\cos\frac{\beta}{2}+\frac{l}{2}\right)\right]$$

but $r\cos\frac{\beta}{2}-\frac{l}{2}=$ the lap on the steam side $=x$;

$$\therefore\ \cos\theta=\cos\left(\cos^{-1}\frac{x'}{r}+\cos^{-1}\frac{x+l}{r}\right)\ \text{and}$$

$$c=\mathrm{R}-\mathrm{R}\cos\left(\cos^{-1}\frac{x'}{r}+\cos^{-1}\frac{x+l}{r}\right)\quad .\quad .\quad (8)$$

It will be observed, that if the quantity $\frac{x'}{r}$ is negative, the arc of which it is the cosine must be subtracted from 180°.

To find the distance of the piston from the end of the stroke at the time the steam is allowed to escape into the condenser, let θ' = the angle passed through by the crank, and $e=\mathrm{R}-\mathrm{R}\cos\theta'$ = the required distance. The angle θ' will be equal to 180° minus the angle, *w'cu''*, due to the difference between the steam and exhaust lap, and we may suppose it to be composed of the three angles *ucd*, BCD=90°, and D*cu''*, then

$$\cos\theta'=\cos\left[\sin^{-1}\left(r\cos\frac{\beta}{2}-\frac{l}{2}-(x-x')\right)+90^\circ+\cos^{-1}\left(r\cos\frac{\beta}{2}+\frac{l}{2}\right)\right]$$

or, substituting the value of $r\cos\frac{\beta}{2}-\frac{l}{2}$ as before,

$$\cos\theta' = \cos\left(\sin^{-1}\frac{x'}{r}+90^\circ+\cos^{-1}\frac{x+l}{r}\right)$$

and

$$e = \mathrm{R} - \mathrm{R}\cos\left(\sin^{-1}\frac{x'}{r}+90^\circ+\cos^{-1}\frac{x+l}{r}\right) \quad . \quad . \quad (9)$$

These two values, c and e will give the mean points of cushioning and exhausting. As the obliquity of the eccentric-rod will slightly affect these values, the result will be still more accurate, if, in (7), we subtract v from x' for the outward stroke and add v to x' for the inward stroke, and in (8) add v to x' for the outward stroke and subtract it for the inward stroke. The reason for this will become apparent by inspecting the diagram. The equations will now become

$$c = \left\{ \begin{array}{l} \mathrm{R} - \mathrm{R}\cos\left(\cos^{-1}\frac{x'-v}{r}+\cos^{-1}\frac{x+l}{r}\right)\text{outward stroke} \\ \mathrm{R} - \mathrm{R}\cos\left(\cos^{-1}\frac{x'+v}{r}+\cos^{-1}\frac{x+l}{r}\right)\text{ inward stroke,} \end{array} \right\} \quad . \quad (10)$$

and

$$e = \left\{ \begin{array}{l} \mathrm{R} - \mathrm{R}\cos\left(\sin^{-1}\frac{x'+v}{r}+90^\circ+\cos^{-1}\frac{x+l}{r}\right)\text{outward stroke} \\ \mathrm{R} - \mathrm{R}\cos\left(\sin^{-1}\frac{x'-v}{r}+90^\circ+\cos^{-1}\frac{x+l}{r}\right)\text{ inward stroke} \end{array} \right\} \quad (11)$$

By substituting the value of $2\mathrm{R}-c$ or $2\mathrm{R}-e$ for L in (7), the value of s may be found for c or e; then $c-s$ and $e-s$ will be the true points for the outward stroke, and $c+s$ and $c+s$ the true points for the inward stroke.

The manner of proceeding to design a slide-valve will then be as follows:—

1st, After having determined the width of the steam-port opening and the point of cutting off, find the approximate radius of the eccentric by (2).

2d, Determine, as nearly as possible, the length of the eccentric-rod, and find by (5) the value of the versed sine of the angle which it forms with the horizontal line when the valve is on the point of opening.

3d, Assume the amount of lead desired, and then find the true radius of the eccentric by (4).

4th, Find the lap by (6).

5th, Assume the amount of exhaust lap, and determine the mean points cushioning and exhausting by (8) and (9). If these are not satisfactory, the exhaust lap may be varied until they are.

If it is desired to construct an indicator card, or if the actual points of cutting off, cushioning and exhausting are required for each stroke, the values of c and e can be determined from (10) and (11), and the variation caused by the obliquity of the connecting-rod can be calculated from (7) by substituting for L the previously found mean distance of both strokes, passed over by the piston in reaching the point in question. These variations must be *added* to the distances from the *commencement* of the stroke, already found, for the outward stroke, and *subtracted* for the inward stroke.

Equations (8), (9), (10), and (11) might be transformed so that they could be worked without the aid of a table of natural sines, but they are now in their simplest form.

The diagram shows a method of ascertaining all the required values geometrically, after the radius of the eccentric and the lap of the valve are found.

It is practically desirable to keep the stroke of the valve as small as possible, and it is principally for this reason that the obliquity of the eccentric-rod has been taken into account. For the same purpose, the steam-ports should be made as narrow as possible, the requisite area being made up by length. This will cause more resistance to the passage of the steam, but the area can be slightly increased to compensate it.

As the steam is not required to enter the cylinder as quickly as it should leave it, the steam side of the valve should only uncover about three-fourths of the width of the port. The exhaust side should always give the full opening, which it will do if the exhaust lap is properly proportioned to the steam lap. The *opening* of the port on the steam side of the valve, is what must be used with the foregoing formulas. A good rule for finding the area, in square inches, of the steam-port, is, to multiply the square of the diameter of the cylinder, in inches, by the velocity of the piston in feet per minute, and divide by 4000.

It might, also, be observed, that the length of the link, from centre to centre, should be at least three times the stroke of the valve, and that the best radius for its centre line is the distance from the centre of the pin to the centre of the eccentric.

To show more plainly the method of computation, it is deemed expedient to subjoin the following

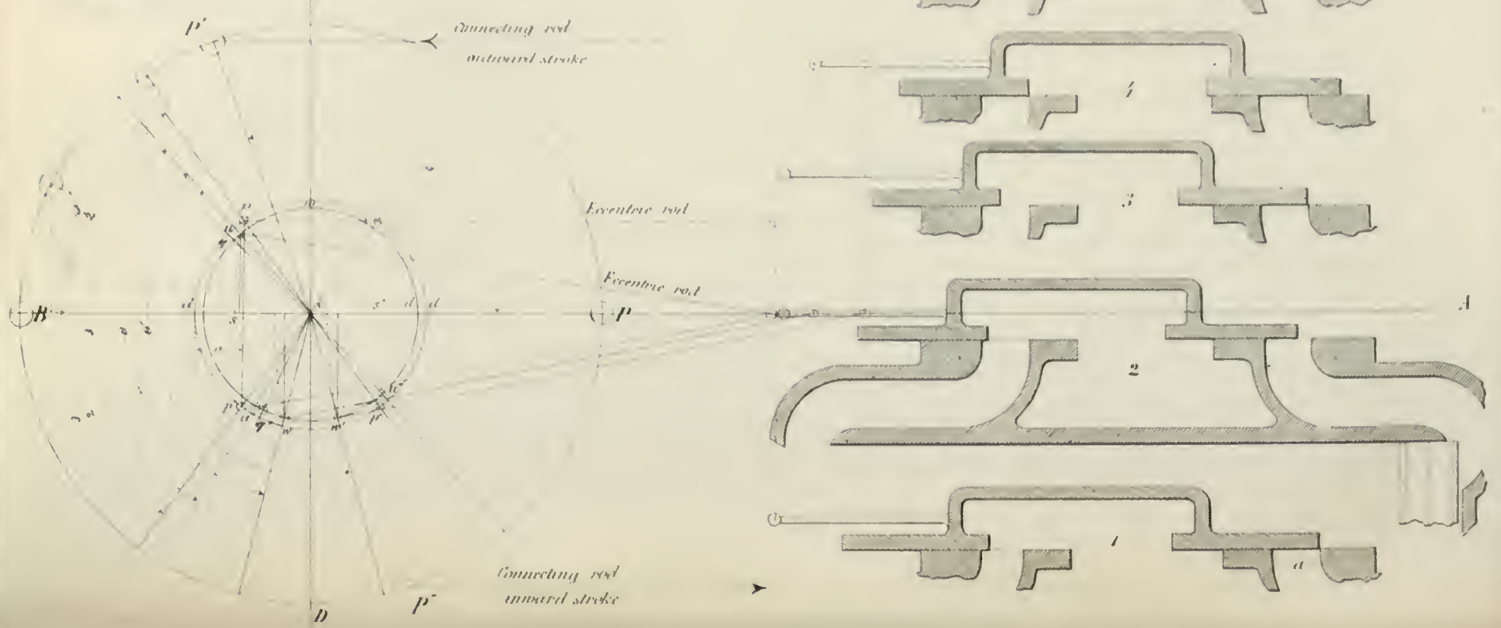
EXAMPLE. The stroke of the steam piston of an engine is 12 inches; the mean point of cutting off 8 inches from the commencement; the width of the steam-port $\frac{1}{2}$-inch; the length of the connecting-rod 30 inches; the lead $\frac{1}{8}$-inch; the length of the eccentric-rod 20 inches, and the proposed exhaust lap one-half the steam lap. Required the radius of the eccentric; the lap of the valve; and the points of cutting off, cushioning, and exhausting, for both strokes of the piston.

$$\frac{\cdot 5}{1-\sqrt{\frac{12-8}{12}}}=1\cdot 183=\text{approximate radius of eccentric,} \quad . \qquad (2)$$

$$20-\sqrt{20^2-\frac{8\times 1\cdot 183^2}{12}}=\cdot 024=\text{versed sine of arc of eccentric-rod} \quad (5)$$

DIAGRAM OF SLIDE VALVE MOTION

The larger of the inner circles represents the path of the center of the eccentric before the radius is modified by the lead and the obliquity of the eccentric rod. Section 1 is the corresponding valve



$\dfrac{\cdot5-\cdot0625+\cdot024}{1-\sqrt{\dfrac{12-8}{12}}}=1\cdot0919=$ true radius of eccentric, . . (4)

$1\cdot0919\sqrt{\dfrac{12-8}{12}}-\cdot0625=\cdot5679=$ lap, . . . (6)

$6-6\cos\left(\cos^{-1}\dfrac{\cdot2839-\cdot024}{1\cdot0919}+\cos^{-1}\dfrac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos$ $(76^\circ\ 14'+50^\circ\ 37')=6-6\cos 126^\circ\ 51'=6-6\times\cdot5997=2\cdot4=$ cushion for outward stroke independent of connecting-rod, . (10)

$6-6\cos\left(\cos^{-1}\dfrac{\cdot2839+\cdot024}{1\cdot0919}+\cos^{-1}\dfrac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos(73^\circ\ 37'$ $+50^\circ\ 37')=6-6\cos 124^\circ\ 14'=6-6\times\cdot5625=2\cdot62=$ cushion for inward stroke independent of connecting-rod, . . (10)

$6-6\cos\left(\sin^{-1}\dfrac{\cdot2839+\cdot024}{1\cdot0919}+90^\circ+\cos^{-1}\dfrac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos(16^\circ$ $23'+90^\circ+50^\circ\ 37')=6-6\cos 157^\circ=6-6\times\cdot9205=\cdot477=$ exhaust for outward stroke independent of connecting-rod, . . (11)

$6-6\cos\left(\sin^{-1}\dfrac{\cdot2839-\cdot024}{1\cdot0919}+90^\circ+\cos^{-1}\dfrac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos$ $(13^\circ\ 46'+90^\circ+50^\circ\ 37')=6-6\cos 154^\circ\ 23'=6-6\times\cdot9017=\cdot589=$ exhaust for inward stroke independent of connecting-rod, . (11)

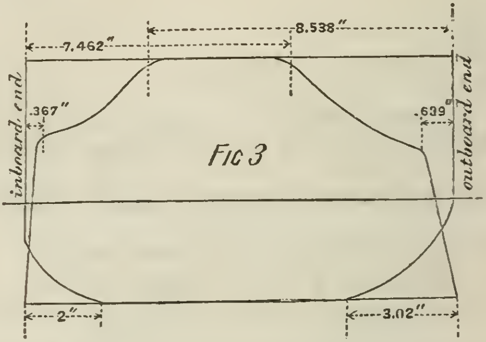
$30-\sqrt{30^2-[6^2-(6-8)^2]}=\cdot538=$ variation in cut-off caused by connecting-rod, ... (7)

$30-\sqrt{30^2-[6^2-(6-9\cdot49)^2]}=\cdot4=$ variation in cushion caused by connecting-rod, ... (7)

$30-\sqrt{30^2-[6^2-(6-11\cdot47)^2]}=\cdot11=$ variation in exhaust caused by connecting-rod, ... (7)

ANSWER :—Radius of eccentric, 1·09 inch; lap of valve, ·56 inch; point of cutting off on outward stroke, 8+·538 = 8·538 inches from commencement; point of cutting off on inward stroke, 8 — ·538 = 7·462 inches from commencement; cushion on outward stroke, 2·4—·4 = 2 inches from end; cushion on inward stroke, 2·62+·4 = 3·02 inches from end; exhaust on outward stroke, ·477—·11=·367 inch from end; exhaust on inward stroke, ·589+·11 = ·699 inch from end.

Fig. 3, constructed from these dimensions, represents the indicator diagram that would be given by an engine having proportions according to this example.



These computations can be made in an hour, and the results will be more accurate than can be obtained by an expensive wooden model.

Navy Department, Washington, Sept. 5th, 1864.

For the Journal of the Franklin Institute.

The Tunnel to Supply the City of Chicago with Water.

MR. EDITOR:—

I had the pleasure a few days since of visiting the Chicago Tunnel, which is intended to supply the city with water. The public may be pleased to know that it is progressing very favorably. I descended the land shaft and passed along the tunnel about 600 feet, to where the miners were at work. The earth which they were removing was a very stiff clay. The soil thus far has been the very best kind for securing permanency of the work. The tunnel is nearly circular, the horizontal diameter being 5 feet and the vertical 5 feet 2 inches. It will be two miles long when completed. The vertical shaft is nearly seventy feet deep. Thirty feet of the upper end is protected by cast iron cylinders which are seven feet in diameter. The remaining forty feet passes through clay and is bricked.

When worked to its utmost capacity it is thought that it might supply 1,000,000 inhabitants. According to contract it is to be completed by November 1st, 1865. D. V. W.

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, May 2, 1864.]

From the Lond. Practical Mechanic's Journal, May, 1864.

During the last month 311 engines have been examined and 418 boilers, 20 of the latter being examined specially, and 4 of them tested with hydraulic pressure. Of the 418 boiler examinations, 333 have been external, 11 internal, and 74 thorough. In the boilers examined, 255 defects have been discovered, 4 of them being dangerous. Details of these will be found in the following statement.

Furnaces out of shape 4 (1 dangerous); fracture 6 (2 dangerous); blistered plates 6; corrosion 24; grooving-internal 5; external 6; feed apparatus out of order, 6; water gauges ditto, 14; blow-out apparatus ditto, 16; fusible plugs ditto, 1; safety valve ditto, 2; pressure gauges ditto, 18; without water gauges ditto, 3; ditto pressure gauges, 39; blow-out apparatus, 35; ditto feed back pressure valves, 68; cases of over-pressure, 1 (dangerous); deficiency of water, 1.

Of some of the defects enumerated above, a few particulars may be given:—

Furnace plates out of shape.—One of these cases occurred to an internally-fired boiler, simply from the want of strengthening hoops, and another to a boiler externally-fired, and of the class termed French or Elephant, having two horizontal tubes of 2 feet diameter, completely filled with water, and set immediately over the fire, and one upper cylinder of 4 feet 6 inches diameter, partly filled with water and partly filled with steam, connected to the two lower ones. One of these lower tubes gave way over the fire, the plate bulging downwards into a cup-shape, and the metal, which was originally three-eighths of an inch in thickness, being wasted away to three-sixteenths. The feed water was very sedimentary, and in this construction of boiler, as indeed in all those fired underneath, the sediment is apt to lodge in the most dangerous part, viz.—immediately over the fire. Instances of the danger attending the plan of external firing are constantly occurring.

It may be added that the plates of the lower cylinders in these Elephant boilers frequently become burnt away, in consequence of the steam being confined in the lower chamber, through imperfect circulation of the water. Also these boilers are extravagant in their consumption of fuel, and very dependent on the integrity of their brick-work setting, which is found to be a frequent source of trouble and expense; while the arrangement of fittings, such as water-gauges, &c., is complicated and inconvenient.

Fracture.—One of these cases occurred to an internally-fired double-story boiler, somewhat similar in construction to those of the Elephant class, with the exception that it had but one cylinder or trunk beneath the main chamber instead of two, while that was internally instead of externally-fired. The fracture took place at a transverse seam of rivets at the crown of the furnace immediately over the fire, the plates being buckled out of shape, and the rivets shorn through. It would appear that this injury arose from two causes. First:—Compression due to the expansion of the plates, consequent on their over-heating from the imperfect circulation of the water, to which these double-story boilers are always liable. Second:—Contraction, consequent on the injudicious practice of cooling the boilers too rapidly for cleaning, by the introduction of cold water. This plan of suddenly cooling hot boilers with cold water cannot be too carefully avoided. Few practices convert a new boiler into an old one more rapidly.

A second case occurred to a boiler only recently placed under the charge of the Association, which was of the ordinary "Lancashire" construction, internally-fired, and set upon a mid-feather, and had failed at the transverse seams at the bottom of the shell, the fracture being what is commonly termed "seam-rending." In consequence of this, the boiler, although a new one, had been frequently repaired, in addition to being re-bottomed, and upwards of £200 expended upon it in little more than a year. These repairs, however, were ineffectual, the "seam-rending" recurring immediately on the boiler being set to work. This was due to the injudicious manner in which the feed-water was introduced, viz: at the opening for the blow-out at the bottom of the boiler. The simple heating of the feed, or its introduction near the surface of the water, through an internal perforated pipe, so as to disperse it and prevent its lying in a dead mass along the bottom of the boiler, would have prevented the injury.

Grooving: Internal. —Two cases of serious grooving or channeling have been met with at the flat crown plates of steam domes, consequent on their frequent buckling. A source, frequently of inconvenience, and sometimes of danger, would be avoided by dispensing with steam domes altogether.

Grooving: External.—Six cases have been met with during the past month which occurred at the bottom of multitubular boilers, from the leakage of the transverse seams. This leakage was due to the unequal contraction of the plates, produced by the imperfect circulation of the water in these boilers. Some of these channels were eaten nearly through the plate.

Cases of Deficiency of Water.—One met with during the last month was due to the watchman's keeping up the fires within the boiler without noticing that the water was out of sight in the gauge-glass. It appears that the boiler lost its water through leakage at the back pressure-valve. Owing, however, to the inlet being nearly as high as the furnace crowns, the damage done to them was but trifling. Where the feed is introduced a little above the level of the furnace crowns, they cannot be laid bare by the water either being drained or syphoned out.

Another case of deficiency of water, which occurred a short time since, arose from the failure of the sough, into which the waste pipe for the blow-out at the bottom of the boiler discharged. On the attendant's opening the blow-out, the brick-work blew up, and he was unable to get within reach to close the tap, so that the furnace crowns were laid bare and became overheated. In consequence, however, of the boiler being fitted with a low-water safety-valve which let off the pressure of the steam on the water's falling below the proper level the injury to the furnace crowns was very trifling. Had blowing-out from the surface of the water been adopted instead of from the bottom, the furnace crowns could not have been laid bare.

These two cases of injury afford an illustration of the advantage of the plan recommended by the Association of introducing the feed, as well as blowing-out, at the surface of the water, and it is thought that it would be well were this arrangement generally adopted.

Explosions.—No. 5 Explosion—the consideration of which was deferred from the last to the present report—was of a very serious character, resulting in the death of 13 persons, and in injury, in some cases, very severe to 10 others. It took place at an iron works, the boilers of which were not under the charge of this Association.

The explosion was compound, two boilers exploding together. These compound explosions, though remarkable, are not singular, and particulars of two have already been given in previous monthly reports.

In the present instance the series consisted of seven, in communication one with the other, and extended from west to east, the boilers themselves lying north and south, having their furnaces at the south end, and the chimney at the north. At the west end of the series stood the engine-house, close to which No. 1 boiler lay, and it was No. 1 and No. 2 that exploded. All the boilers were externally-fired, No. 6 and No. 7 being of plain cylindrical egg-ended construction, and the remainder including the exploded ones, being cylindrical in the shell, with one flue tube, and having rounded ends, not truly hemispherical, but such as are known in the district by the term of "bull-dog." The boilers were set upon mid-feathers, and fired underneath, the flames passing in the first instance along the left-hand side of the wall, then returning on the other, and finally passing through the internal flue tube to the chimney.

The rents in both the exploded boilers had taken place at the furnace or south end, and a portion of the shell containing three or four rings of plates had severed itself in each instance from the remainder. The main portion of the shells had flown northward, that of No. 2 to a distance of 80 yards, and that of No. 1 to 30 yards. The former had fallen beyond the works, but the latter immediately upon one of the puddling furnaces, knocking down the chimney, and demolishing the roof of the building. A fragment of the firing end of No. 1 had been thrown in a north-easterly direction to a distance of 130 yards. The engine-house, a massive stone building, three stories high, adjoining No. 1 boiler was unroofed, the whole of one side thrown down, and the remaining walls bulged and shaken. The bed of the boilers was completely torn up, and bricks and pieces of stone scattered in every direction. From these missiles many of the deaths occurred. One at a distance of about 105 yards, another at 80 yards, a third at seventy yards, and a fourth at 30 yards; while one person, though fortunately not killed, was struck and seriously hurt when standing 100 yards off; and I observed an edge stone, which, though it had fallen at a distance of 200 yards from the boilers, had yet cut its way into the ground with sufficient force to have killed any one it had struck. Those injured by the flight of the boilers themselves were comparatively few; while as many as six men were killed in the engine-house, by the steam and hot-water that played in upon them, from the boilers remaining at work, after the explosion of Nos. 1 and 2, although the intervening steam pipe for a length of about 40 feet had been carried away.

Destructive and complicated as the results of the explosion were, the cause is very simple, as indeed it is in every case.

It appears that the water with which the boilers were fed was highly corrosive, a portion of it being derived from some coal workings a short distance up the valley in which the iron works were situated. This water had eaten into the surface of the plate in a most remarkable manner, and I have never seen any so severely affected. A considerable portion of the inside of the boilers, presented an appearance very similar to a honey-comb, the cells running closely one into another, and in many cases penetrating so deeply into the body of the plate as only to leave a mere film of metal about one-sixteenth of an inch thick. The corrosive action was specially active at the overlaps, where it eat away a deep channel, completely undermining the edge of the plate, and in some cases penetrating as far as the body of the rivet. This channeling was quite independent of mechanical action, being as severe at the transverse as at the longitudinal seams. The plates which were most affected by this corrosive action were those nearly over the fire-bridge at the bottom of the boiler, and in the vicinity of the feed-inlet. Those on the left-hand side of the mid-feather wall, were more severely attacked than those on the right, in consequence, as it would appear, of the higher temperature, since the flames from the furnace passed on that side in the first instance. The intensity of the action diminished towards the back end of the boilers, and its effect upon No. 1 did not appear to be precisely similar to that on No. 2, the latter being deeply honey-combed, and the former eaten away more evenly. In No. 2 there could not have been less than 50 square feet of honey-combed surface.

With regard to the compound nature of the explosion: That the boilers did not burst simultaneously, but that the explosion of one caused that of the other, is attested by the fact that those at the various works at the time heard two distinct reports. The primary rent, it appears, was at a longitudinal channel immediately over the fire at the bottom of No. 1, the edge of the plate at this fracture for a length of some feet being reduced to a knife-edge This rent extended longitudinally throughout the first two or three rings of plate, and then assumed at the adjoining ring seam of rivets, a transverse direction running completely round the circle.

For the present month, 3 explosions have to be reported, from which 15 lives have been lost, and also 25 persons injured. Not one of the boilers was under the charge of this Association. The scene of the explosion has been personally visited in each case, and the cause investigated.

No. 6 Explosion was of a very fatal character, twelve persons being killed, and nineteen others injured. It occurred at an iron works, the boilers of which were not under the inspection of this Association. The exploded boiler was of the vertical furnace class, and heated by the flames passing off from three iron furnaces. These flames played in the first instance on the outside of the boiler, then passed through three neck-openings into a central internal descending flue, and thence by means of a culvert to the chimney.

No. 7 Explosion, which resulted in the death of one person and in injury to six others, occurred at a saw-mill, and was due to the collapse of a furnace flue, and is an illustration of the necessity of strengthening all such flues with hoops or other suitable means.

The boiler which was not under the inspection of this Association, was of Cornish construction, 20 ft. in length, and about 4 ft. 6 ins. diameter in the shell. The furnace tube was slightly oval, very likely unintentionally so, through imperfect workmanship, and measured at the furnace mouth 2 ft. 6 ins. vertically, and 2 ft. 4½ ins. horizontally. The thickness of the plates in the shell was three-eighths, and in the furnace tube five-sixteenths of an inch.

The furnace tube had collapsed from the fire-bridge to the end of the boiler. These tubes usually collapse vertically the crown coming down, but in this case the movement had been horizontal, the two sides coming together. The fire-bridge had arrested the collapse at that part, and the crown-plates at the furnace end, which showed no signs of having been overheated, remained uninjured. The tube had severed at one of the ring seams of rivets midway between the fire-bridge and the end of the boiler, and the back end plate had been torn away from the shell; while the main portion of the boiler was thrown forwards across the stoke-hole, and the fragments blown in the opposite direction.

The damage done to the surrounding property was considerable, not so much perhaps from the peculiar violence of the explosion, as from the fact of the boiler being surrounded by buildings, many of which were dwellings. The works in which the explosion took place were leveled to the ground and reduced to a heap of ruins, while the surrounding houses were so damaged that they presented the appearance of having been bombarded, and some of them it is reported, had to be condemned on the following day, by the district surveyor, on account of their dangerous condition.

No. 8 Explosion, which resulted in the loss of two lives, afford an additional illustration of the danger so often pointed out in these reports, as attendant upon the use of externally-fired boilers, especially when they are fed with sedimentary water and not provided with efficient blow-out apparatus.

The explosion occurred at an iron works, the boilers of which were not under the inspection of this Association. The boiler in question was No. 3 in a series of eight, working side by side, and connected together both by the steam pipe and feed pipe. They were of plain cylindrical egg-ended construction, externally-fired, and set with a flash flue: their length being 40 ft., their diameter 5 ft., and thickness of plate three-eighths of an inch.

The boiler gave way at one of the transverse seams of rivets situated about 8 ft. from the front end, and very near to the fire-bridge.

The rent commenced at the bottom, and then continued throughout the entire circle of rivets, severing the shell completely into two sections, which flew in a straight line in opposite directions. Judging from objects struck in its flight, the furnace end of the shell must have assumed a considerable angle of elevation immediately on quitting its seat. Two boilers on each side of the exploded one were dislodged from their original position and thrust laterally. Had not the flue below them been unusually roomy, and thus afforded considerable vent for the steam and water which escaped from the rent, it is more than probable, that the explosion would have spread to the sister boilers as it has done in other cases, and perhaps have involved the whole series.

This explosion was not due to shortness of water, as an examination of the condition of the plates clearly showed, neither was it due to the excessive pressure.

Having now given the principal facts of the preceding explosions, a few general remarks, in conclusion, may not be out of place.

It appears that, during a period of scarcely three months,—*i.e.*, from the commencement of the present year, up to March 25th, the day to which report is made up,—there have occurred eight explosions, from which 29 persons have been killed, and 42 others injured; while five persons connected with the management of these boilers have been committed for manslaughter.

It cannot have escaped attention how extremely simple the cause of all these explosions have been; in three cases, the choking of the feed pipes through frost; in two others, wasting of the plates to the thickness of a sheet of paper; and in the sixth, the mal-construction of the boiler.

Iron works appear to maintain their position at the head of the list, both for the number and fatality of their explosions, and, under these circumstances, it may not be unwise to re-consider the policy now generally adopted at these works, viz: that of employing the more primitive description of engineering arrangements, in preference to those of modern character, and now widely adopted in other branches of industry. It is often argued that the rough class of labor obtainable at iron works necessitate the continuance of primitive and rough mechanical appliances; an argument, it is thought, that admits of question. True economy in engineering matters is only to be found in the employment of the best material and workmanship. The continued use of inferior and rough boilers, tends to perpetuate inferior and rough workmen; while on the other hand, the use of a superior description, would stimulate and raise them; and should the statement which is sometimes made be correct, that iron works will not supply workmen of the requisite stamp, nothing could be easier than to import them from other departments of engineering. Where boilers of a superior class have been adopted, they have been found to be productive of economy in working, as well as of human life, and there can be no reason to doubt that these advantages would follow their adoption at iron works.

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, Nos. 214 and 215.

(Continued from page 176.)

4. *Invention of the Air Pump.*—About ten years after the discovery of the pressure of the air, Otto Von Guericke,* a burgomaster of Magdeburgh, made a number of experiments, with a view of obtaining a larger vacuous space than that which could be procured by Torricelli's method; for, although the behavior of certain bodies in a vacuum had been tried by introducing them into the Torricellian vacuum, this mode of experimenting was obviously attended with numerous difficulties, and but few substances could be placed in a vacuum obtained by such a process.

Guericke conceived that he could effect his object by completely filling a vessel with some substance which could be removed without allowing air to take its place. He, accordingly, filled a wooden cask with water, and fitted into it a syringe, all other parts being closed as securely as possible; he then pumped out the water, expecting that a vacuum would be produced in the cask, but air readily entered, by the crevices, to occupy the space which the water left.†

Having previously observed, however, that air could be made to expand to any extent, he determined to cause air to leave a vessel, in virtue of that property, and to prevent its return by external means. In order to effect this, he took a copper globe, and fitted into it a syringe; when the pumping was commenced, the piston worked somewhat easily, but became more and more difficult to raise, and in a few minutes the globe was compressed with a loud noise.

Guericke next procured a glass globe, furnished with a straight brass tube, containing a stop-cock, by means of which, free communication with the interior of the globe could be established or prevented; to the lower part of the tube, and inclined to it at an angle of 45°, a syringe, having a solid piston, was adapted. Near the lower end of the syringe, a lateral valve opening outwards was placed, and at the juncture of the syringe with the tube there was a second valve, opening into the syringe. The valve and stop-cock were immersed in water to prevent air from leaking into the apparatus.‡ It is obvious that, on raising the piston of such an air-pump, the air in the globe will expand, open the lower valve, and escape into the body of the syringe, while the lateral valve will be kept closed by the pressure of the air; and, on depressing the piston, the lower valve will close, and the lateral valve will open to allow the air in the body of the syringe to escape. A good vacuum could be obtained by this instrument, but it required a great deal of pumping to produce it.

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* Born, 1602. Died, 1686.

† See "Ottonis de Guericke Experimenta Nova (ut Vocantur) Magdeburgica de Vacuo Spatio." Amstelodami. 1672.

‡ See "Mechanica Hydraulico Pneumatica," by Gaspar Schottus. 1658. Also, "Technica Curiosa," (1664), by the same author.

Soon after the invention of the air-pump, Guericke constructed the "Magdeburgh hemispheres," a full account of which, Schottus informs us, was first given to him in July, 1656. In Guericke's "Experimenta Nova," there is a picture which conveys to the mind a good idea of the enormous pressure of the air. In the centre of this picture we observe a pair of large Magdeburgh hemispheres, to each of which eight horses are attached, and, although most evidently urged to their uttermost by men, who flourish aloft gigantic whips, they are unable to effect the division of the sphere.

Among other experiments made by Guericke with his air-pump, we may mention the following:

1. A sparrow placed in the receiver died on exhausting.

2. A candle was extinguished in an exhausted receiver, because, says Guericke, *fire consumes air*.

3. Water was found to rise to a great height in an exhausted tube, and when the exhausted receiver of the pump was opened under water, water violently entered, and almost filled it.

4. A vessel exhausted of air was found to be considerably lighter than before exhaustion.

5. A bell made to ring in an exhausted receiver was inaudible until air had been admitted.

Kircher* had previously tried this experiment in the Torricellian vacuum by ringing a bell, the clapper of which was of iron, and was moved by the alternate approximation and withdrawal of a magnet. Kircher states that the sound was distinctly audible, from which we may infer that he suspended his bell within the tube by means of some substance capable of transmitting the vibrations to the tube.

Guericke did not believe the air to be elemental, and considered it incapable of being converted into water.

The following is his definition of air.

"Aer est nihil aliud quam expiratio aut effluvium, aquarum, terrarumque, et aliarum rerum corporearum."

The science of Pneumatics, which took its rise immediately after the invention of the air-pump, was greatly extended in England by some of that band of experimental philosophers which arose shortly after the death of Bacon. Foremost in that band, if not the very leader of it, was Robert Boyle,† a man who, despising all previous philosophies, applied himself vigorously to carry out the ideas of Lord Bacon. Boyle had the greatest reverence for the "Novum Organum," and there could not be a mind better adapted than his for putting in practice the "new philosophy." He was the very man that was wanted, at a time when speculative philosophy, unaided by experiment, was being changed for experimental philosophy, aided by inductive reasoning.

"But our hope of further progress in the sciences," writes Bacon, in the "Novum Organum,"‡ "will then only be well-founded when numerous experiments shall be received and collected into natural history, which, though of no use in themselves, assist materially in the discovery of causes and axioms, which experiments we have termed, enlightening, to distinguish them from those which are profitable. They possess this wonderful property and nature, that they never deceive or fail you; for being used to discover the natural cause of some object, whatever be the result, they equally satisfy your aim by deciding the question."

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* Born, 1601. Died, 1680.

† Born, 1626. Died, 1691.

‡ Book 1, Aph. 99.

Now, Boyle was the man to thus promote the welfare of the sciences; he was not one to cultivate them for profit to himself, or for renown, but he chose rather to study science for the pure love of the thing; and such men only have a right to bear the title of Philosopher, and by them alone is science permanently benefited. Bacon was the architect who planned out a glorious structure, and Boyle was one of the master-builders of his day; and he laid the foundation of a part of that building so securely, that it still endures; and although now and then we have to remove a brick which has fallen to decay, we still continue to build on that foundation, and the workmen increase, and the building rises rapidly,—let us hope it may be soon roofed in.

The writings of Boyle extend over a period of thirty years; they are excessively prolix, but with so few previous experiments on most of the subjects, it was difficult to avoid describing the most trivial effects: we must remember, moreover that he wrote of phenomena hitherto almost unknown to the human mind, and we must overlook the tediousness of the accounts of his experiments, which, although they appear very detail to us, were probably not too much so for his contemporaries.

Boyle's first work on the air appeared in 1661, and was entitled "New Experiments, Physico-Mechanical, Touching the Spring of the Air and its Effects; made for the most part in a new Pneumatical Engine." The entire work is written in the form of a letter to Lord Dungarvan, and is dated December 20, 1659.

So soon as Boyle heard of the invention of Otto Von Guericke's air-pump, he determined to construct one which should be less cumbrous, and should not require to be placed in a vessel of water. He mentioned his wish to his friends, Hooke and Greatorex, and, after several unsuccessful trials, Hooke made for him an air-pump which was considered to be superior to Guericke's. It consisted of a hollow vertical cylinder of cast brass, fourteen inches long and three inches internal diameter, the lower end of which was open, and the upper closed, with the exception of a small orifice in the line of the axis of the cylinder, and a tapering, lateral orifice, into which a brass stopper, serving as a valve, fitted air-tight. A solid piston, to which vertical motion was given by means of a rack and pinion, worked within the cylinder. The receiver* was a glass globe capable of containing thirty wine quarts, fitted with a stop-cock, the shank of which was cemented into the upper orifice of the cylinder; and in that part of the globe opposite the stop-cock there was a circular-lipped opening four inches in diameter, to which a brass ring (diminishing the diameter of the opening to three inches) was cemented; the ring was closed by an "exquisitely-ground" brass plate, containing a half-inch circular orifice closed by a brass stopper, which could be turned, when the receiver was exhausted, without admitting air. The whole apparatus was arranged vertically, so that it was terminated above by the opening of the receiver for the introduction of bodies to be experimented upon, and below by the open orifice of the brass cylinder.

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* No one can have failed to observe how singularly inapplicable the name of "*receiver*" is to that part of an air-pump which it is desired to *empty* as completely as possible; the name was given by Boyle, who, in speaking of the vessel to be exhausted, says, "which we, with the glass-men, shall often call a receiver, for its affinity to the large vessels of that name, used by chemists."

Before commencing to exhaust the receiver, the piston and all the joints were well oiled, the piston pushed to the top of the cylinder, and the stop-cock and lateral valve closed; the piston was then drawn to the bottom of the cylinder, and the stop-cock opened to allow air to rush from the receiver into the vacuous space; the stop-cock was now closed, the valve opened to allow of the exit of the air in the body of the cylinder, and the piston again forced to the top of the cylinder; the same process was repeated until the receiver was exhausted.

I do not consider this first air-pump of Boyle's by any means so ingenious a contrivance as that of Otto Von Guericke. The arrangement of the apparatus was indeed different, but the method of exhaustion was the same, more clumsily effected; the stop-cock of Boyle's pump played the same part as the lower valve of Guericke's, and the lateral valve of the former the same as the lateral valve of the latter, with this disadvantage, however, that the valves of Boyle's pump had to be worked by hand, while those of Guericke's were worked by the air.

Some of the more important of the forty-three experiments detailed in the treatise we are considering, are given below:

In the first experiment Boyle explains by what means the receiver of his air-pump is exhausted, and he affirms that most of the experiments which he is about to relate may be explained by bearing in mind the fact, "that there is a spring or elastical power in the air we live in."

He compares the air to a fleece of wool, or a dry sponge,* or says we may admit Des Cartes' theory, that the air is made up of innumerable flexible particles, which are so moved about by the motion of the celestial matter in which they swim, that each particle keeps those around it at a distance. Boyle prefers to believe that the air itself is elastic, rather than to admit Des Cartes' theory.

The height of the atmosphere, according to Kepler, is only eight miles, but Ricciolo makes it probable that it extends to a height of fifty miles. "Can we not, therefore, well imagine," writes Boyle, "that the weight of a column of air of such a height will keep 'the little springs' of the air bent, and that when that weight is removed they will straighten themselves.

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* Hero, of Alexandria, who was acquainted with the elasticity of the air, compares it to horn shavings, or a sponge.

Pascal, as we have seen in the previous paper, compared the air to wool, a favorite simile of Boyle's throughout this work, but whether taken from Pascal or not it is difficult to say. Pascal's treaties "On the Weight of the Mass of Air." was not published till 1663, but Boyle may have heard of the comparison from some one who had conversed with Pascal.

Experiment 2. When a stoppered receiver was exhausted of air, great force was found to be necessary to remove the stopper.

From the length at which this experiment is detailed it would appear that Boyle had not heard of the Magdeburgh hemispheres, although they were invented several years previously.

Experiment 4. A half-blown bladder placed in the receiver became fully expanded on exhausting.

This experiment had been previously tried by Roberval, by introducing a nearly empty carp's bladder into the Torricellian vacuum.

Experiment 5. When the exhaustion was long-continued the bladder burst; a partially-filled bladder also burst when held near a fire.

Experiment 6. In order to determine to what extent a known quantity of air could be made to expand, when pressure was removed from it, Boyle took a phial containing four or five drachms, tied an empty lamb's bladder over its mouth, and placed it in the air-pump receiver; exhaustion was continued until the bladder was fully expanded, and by comparing the capacities of the phial and bladder, the air was found to have expanded to nine times its original bulk.

This mode of experimenting was evidently borrowed from Bacon, who, in the 40th Aph. of Book 2 of the "Novum Organum," describes an experiment which he made in order to determine the space occupied by the vapor produced from a liquid occupying a known space; in other words, the ratio between the spaces occupied by a substance when in the liquid and gaseous states. This experiment, although it could only give most fallacious results, is well worthy of record, inasmuch as it was made at a time when experiments of any kind were rare, and quantitative experiments most rare.

Bacon filled an ounce phial, of known weight, with the lightest liquid with which he was acquainted (spirits of wine), weighed, to determine the amount of spirit taken, and tied an empty bladder, containing about two pints when full, over the mouth of the phial; the spirit was heated until the bladder was fully expanded, when the heating was discontinued, the bladder pricked to allow the vapor to escape, and then removed from the phial; on re-weighing the phial, the amount of spirit converted into vapor was found, and the space it occupied being compared with the contents of the bladder, it was shown that the spirit of wine occupied in the state of vapor 100 times the bulk it possessed in the liquid form.

But we must return to Boyle's experiment on the expansion of air under diminished pressure. After making the experiment with the bladder and phial, he devised a far more accurate method, which was to place within the receiver a tube, closed at one end, to which a divided scale was attached, and the capacity of which was known. The tube was entirely filled with water, with the exception of the space occupied by a small quantity of air of known bulk, and its lower end was placed in a vessel of water. When pressure was removed, the air within the tube expanded and depressed the column of water, and the amount of expansion was shown by the scale. By this means it was found that air expanded to thirty-one times its bulk, but the tube was too short, and the air still continued to expand, when the experiment had to be stopped. He next made use of a tube three feet long, passed it air-tight through the cover of the receiver, and placed the phial of water, into which its open orifice dipped, within the receiver; a known quantity of air was now found to expand to 152 times its previous bulk; it is obvious, however, that the inaccuracy of the experiment increases with the length of the tube, because, when the exhaustion has proceeded to a certain extent, the column of water will fall, not only on account of the expansion of the air above it, but also because its own weight can partially overcome the pressure of the rarefied air in the receiver.

Experiment 7. A small globe of very thin glass filled with air, was placed in air-tight connexion with an exhausted receiver: on suddenly turning the stop-cock, so as to open free communication with the receiver, the globe was not broken, probably, Boyle says, on account of its shape.

Experiment 8. The helmet of a glass alembic was fitted with a stop-cock, every other part being closed; the shank of the stop-cock was cemented into the upper orifice of the pump-barrel; on exhausting, the helmet was broken.

Experiment 9. A glass tube, open at both ends, was cemented firmly into the neck of a glass phial containing a small quantity of water, so that its lower end reached nearly to the bottom of the phial; the latter was placed in a small receiver, and the tube passed air-tight through the cover; on exhausting, the phial was burst,* because, says Boyle, the inside of the phial had to bear the whole weight of the atmosphere, and there was no corresponding pressure to counterbalance it on the outside. The thinnest glass vessel is not broken in the air, because both the inside and outside are equally pressed; in other words, it is submitted to no pressure at all.

Experiment 10. A lighted candle was placed in the receiver; on exhausting, it was extinguished. When burnt in the closed receiver, without exhausting, it continued alight for a much longer time.

Experiment 11. A wire-basket, filled with ignited charcoal, was suspended in the receiver; on exhausting, the charcoal ceased to glow sooner than when the receiver was not exhausted; but a piece of red-hot iron did not appear to lose its redness sooner in a vacuum than when placed in the closed receiver.

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* The phial burst with such violence that it cracked the air-pump receiver,—a not unfrequent occurrence, however, from other causes; and, as receivers were difficult to procure, Boyle was in the habit of covering the cracks with cement, and afterwards with diachylon plaster. The following is his account of the preparation of the cement:—"The plaster was made of good quicklime, finely poudred, and nimbly ground with a pestle in a mortar,. with a quantity (I know not how much precisely, not having the essays in this place) of scrapings of cheese and a little fair water, no more than is just necessary to bring the mixture to a somewhat soft paste, which, when the ingredients are exquisitely incorporated, will have a strong and stincking smell. Then it must be immediately spread upon a linnen cloath of three or four fingers' breadth, and presently apply'd, lest it begin to harden. But if your Lordship had seen how we mended with it receivers, even for the most subtle Chymicall spirits, you would scarce wonder at the service it hath done in our Pneumatical Glass."

Experiment 12. A piece of lighted match, "such as souldiers use," was placed in the receiver; it was quickly extinguished, and an immense quantity of smoke was produced.

Experiment 13. In order to see whether the match was extinguished for want of air, or because the smoke pressed upon it, and stifled the flame, a partially-blown bladder was introduced with the match, to see if the fumes exerted appreciable pressure, but the bladder expanded quite as readily as when it was placed in the receiver without the match.

Experiment 14. Gunpowder could be fired in an exhausted receiver, and a flint and steel meeting together in collision produced sparks.

(To be Continued.)

On the Spectra of some of the Fixed Stars.

From the London Chemical News, No. 236.

A paper "on the spectra of some of the fixed stars," by W. Huggins, F.R.A.S., and Professor W. A. Miller, was then read. After a few introductory remarks, the authors describe the apparatus which they employ, and their general method of observing the spectra of the fixed stars and planets. The spectroscope contrived for these inquiries was attached to the eye-end of a refracting telescope of 10 feet focal length, with an 8-inch achromatic object-glass, the whole mounted equatorially and carried by a clock-movement. In the construction of the spectroscope, a plano-convex cylindrical lens, of 14 inches focal length, was employed to convert the image of the star into a narrow line of light, which was made to fall upon a very fine slit, behind which was placed an achromatic collimating lens. The dispersing portion of the arrangement consisted of two dense flint-glass prisms, and the spectrum was viewed through a small achromatic telescope with a magnifying power of between 5 and 6 diameters. Angular measures of the different parts of the spectrum were obtained by means of a micrometric screw, by which the position of the small telescope was regulated. A reflecting prism was placed over one half of the slit of the spectroscope, and by means of a mirror, suitably adjusted, the spectra of comparison were viewed simultaneously with the stellar spectra. This light was usually obtained from the induction spark taken between electrodes of different metals. The dispersive power of the apparatus was sufficient to enable the observer to see the line *Ni* of Kirchhoff between the two solar lines D; and the three constituents of the magnesium group at *b* are divided still more evidently.* Minute details of the methods adopted for testing the exact coincidence of the corresponding metallic lines with those of the solar and lunar spectrum are given, and the authors then proceed to give the results of their observations. Careful examination of the spectrum of the light obtained from various points of the moon's surface failed to show any lines resembling those due to the earth's atmosphere. The planets Venus, Mars, Jupiter, and Saturn were also examined for atmospheric lines, but none such could be discovered, though the characteristic aspect of the solar spectrum was recognised in each case; and several of the principal lines were measured, and found to be exactly coincident with the solar lines. Between forty and fifty of the fixed stars have been more or less completely examined; and tables of the measures of about 90 lines in Aldebaran, nearly 80 in α Orionis, and 15 in β Pegasi are given, with diagrams of the lines in the two stars first named. These diagrams include the results of the comparison of the spectra of various terrestrial elements with those of the star. In the spectrum of Aldebaran coincidence with nine of the elementary bodies were observed, viz: sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury; in seven other cases no coincidence was found to occur. In the spectrum of α Orionis five cases of coincidence were found—viz: sodium, magnesium, calcium, iron, and bismuth; whilst in the case of ten other metals no coincidence with the lines of this stellar spectrum was found. β Pegasi furnished a spectrum closely resembling that of α Orionis in appearance, but much weaker; only a few of the lines admitted of accurate measurement, for want of light; but the coincidence of sodium and magnesium was ascertained; that of barium, iron, and manganese was doubtful. Four other elements were found not to be coincident. In particular, it was noticed that the lines C and F, correspond to hydrogen, which are present in nearly all the stars, are wanting in α Orionis and β Pegasi. The investigation of the stars which follow is less complete, and no details of measurement are given, though several points of much interest have been ascertained. Sirius gave spectrum containing five strong lines, and numerous finer lines. The occurrence of sodium, magnesium, hydrogen and probably of iron, was shown by coincidence of certain lines in the spectra of these metals with those in the star. In α Lyræ the occurrence of sodium, magnesium, and hydrogen was also shown by the same means. In Capella sodium was shown, and about twenty of the lines in the star were measured. In Arcturus the authors have measured about thirty lines, and have observed the coincidence of the sodium line with a double line in the star spectrum. In Pollux they obtained evidence of the presence of sodium, magnesium, and probably of iron. The presence of sodium was also indicated in Procyon and α Cygni. In no single instance have the authors ever observed a star spectrum in which lines were not discernible, if the light were sufficiently intense and the atmosphere favorable. Rigel, for instance, which some authors state to be free from lines, is filled with a multitude of fine lines. Photographs of the spectra of Sirius and Capella were taken upon collodion; but, though tolerably sharp, the apparatus employed was not sufficiently perfect to afford any indication of lines in the photograph. In the concluding portion of their paper, the authors apply the facts observed to an explanation of the colors of the stars. They consider that the difference of color is to be sought in the difference of the constitution of the investing stellar atmospheres, which act by absorbing particular portions of the light emitted by the incandescent solid or liquid photosphere, the light of which in each case they suppose to be the same in quality originally, as it seems to be independent of the chemical nature of its constituents, so far as observation of the various solid and liquid elementary bodies, when rendered incandescent by terrestrial means, appears to indicate.

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* Each unit of the scale adopted was about equal to $\frac{1}{1163}$th of the distance between A and H in the solar spectrum. The measures on different occasions of the same line rarely differed by one of these units, and were often identical.

Proceedings of the Royal Society, May 26, 1864.

For the Journal of the Franklin Institute.

On the Parabolic Construction of Ships. By John W. Nystrom.

The following communication has been received by me.

Triest, Austria, June 8th, 1864.

Gentleman:—After having perused your treatise on the Parabolic Construction of Ships, your Pocket Book of Mechanics, 9th edition, and a Lecture on the Parabolic Construction, published in the *Journal of the Franklin Institute*, re-published in the *Artizan* for July, 1864, in London, I take the liberty of sending you the theories that sustain Chapman's hypothesis, in which you have not succeeded; with the hope that you will correct your theory, based on wrong hypothesis, before going forward with your *treatise*, now in progress, and that it really will be brought to perfection; and should these proofs be accepted, I may avail myself of another opportunity to forward some more extensive details on the Parabolic Construction, communicated to my family in the year 1806, from Chapman himself, (sending a work entitled, "Försök till en Theoretisk Afhandling att gifva ett Linie skepp dess râtta storlek ach form. Likaledes för Fregatter och mindre Bevârade Fartyg af F. H. af Chapman Carlscrona, 1806,") which was the result of labors of the latter years of Chapman's life, that he dedicated it, as his *testament* or memorial of what he had been able to contribute to it, to all enlightened *admirers* of this noble science.

In case you would be so very kind as to answer this letter, I beg you to have the kindness, if you do not publish the theoretic part of the Parabolic System in your progressional work, to make me acquainted with it, in order that I may be able to know if I have something still to learn of it.

Very respectfully,

Anthony Panfilli,

Mr. John W. Nystrom, C. E. [COAT OF ARMS] No. 990, Triest, Austria.

I have received half a dozen letters of this kind from ship-builders in Europe, which all seem to agree that I am wrong in my Parabolic Construction; but they do not state why or wherein I am wrong, only refer me to Chapman's formulas. I have answered several communications, and requested them to explain why or wherein I am wrong; and if any light is obtained on the subject, I shall not fail to communicate the same to the Franklin Institute. In the meantime, I shall go on, uninterrupted, with my work on the Parabolic Construction, as I am under the impression that they only suppose I am wrong merely because my hypotheses differ from those of Chapman. I may consider myself a pupil of Chapman's school, because it is on his Parabolic method I have started, and progressed as explained in the *Journal of the Franklin Institute* and in the Lecture at the Polytechnic College, Philadelphia. The first part of that Lecture, which explains the inefficiency of Chapman's hypotheses, has not yet been published, but the following is an extract of the same:

Extracts from the first part of the Lecture on the Parabolic Construction, at the Polytechnic College, Philadelphia, February 11, 1864.

It is proposed to explain to you, this evening, a new method of constructing ships, called the *Parabolic Construction.* It is so called because it is based on the formula for a parabola, whereby all the lines in a ship can be constructed.

The Parabolic System of constructing ships was originated by the celebrated Swedish naval architect, Chapman, nearly a century ago; at which period it was well received among ship-builders, but on account of its then incomplete form, restricting constructors to particular shapes, it was gradually abrogated, until no trace could be found of it, even in works on ship-building. Mr. Chapman hit upon the fortunate idea that the cross-sections of the displacement of a vessel ought to follow a certain progression, in order to present the least possible resistance when moving through the water. He collected a great many drawings of ships of known good and bad performances, and made the following investigation. On each drawing he transformed the cross-sections of the displacement into rectangles of the same breadth as the greatest beam of the load-water-line of the vessel; placed their upper edges in the plan of the load-water-line, by which he found that the under edges of the rectangles formed a bottom, the curve of which were parabolas in ships of known good performances.

Let the accompanying figure, 1, Plate IV, represent a ship with the load-water-line, W, dead-flat cross-section a $\mathfrak{X}$ b, formed into the rectangle $a\ b\ c\ d$, and $i\ \theta\ i$ another cross-section formed into a rectangle $e\ f\ g\ h$, so that the breadth $e\ f$ is equal to $a\ b$; then the line $k\ l\ m$, Fig. 1, Pl. IV, forming the bottom of the rectangles, should be a parabola with the vertex at k, and $k\ o$ the axis of the abscissa.

Mr. Chapman found that the parabola so obtained did not terminate at the stem n, but fell a little short at m. The deviation $m\ n$ was very small in vessels of his days, but in modern vessels it is more considerable, showing that there must be a point of inflection p in the curve. However erroneously we may set out in quest of an object, experience generally leads us towards correct scientific principles. In the case before us, experience has increased the deviation $m\ n$, and we know that inasmuch as nature admits of no physical by-laws, the curve cannot be a plain parabola. It is this increasing deviation $m\ n$ which has led me to investigate the subject more carefully; starting on the principle that the resistance to a body in motion in a fluid, is a function of the square of the sine of the angle of incidence to the motion. Let $a\ b\ c\ d$, Fig. 2, Pl. IV, be a body in motion in a fluid, in the direction $a\ c$; then the resistance to that body is found by experiments to be nearly as the square of the sine of the angle v; the beam $b\ d$ being constant.

From this it appears that the proper progression of the cross-sections should be as the square of the ordinates in a parabola.

Let Fig. 3, Pl. IV, represent a vessel with the dead-flat $\mathfrak{X}$ and stem n. Draw the cross-section $a\ \mathfrak{X}\ b$ and the rectangle $a\ b\ c\ d$, as before described; draw a parabola $k\ l\ n$ of any desired order, terminating at the stem; then the proper progression of the cross-sections should be as the square of the ordinates β. Let $b = 1$, then the ordinates β will be fractions of b, and the square β^2 multiplied by the area of the dead-flat cross-section $\mathfrak{X}$, would give the proper area of the ordinate cross-section θ, or $\theta = \mathfrak{X}\ \beta^2$, Fig. 3, Pl. IV. The line $k\ m\ n$ should then indicate the proper progression of the ordinate to cross-sections θ. The areas $c\ f\ g\ h = i\ \theta\ i$.

Here the Lecture was continued on the properties and formulas for the Parabola, which is not necessary to repeat on this occasion. Arrived to the following formulas, referred to the rectangular co-ordinates half length and breadth of the vessel:

$$(a\ b) = b\left(1 - \frac{y^n}{l}\right) \quad . \quad . \quad 1$$

$$(a\ \theta) = \mathfrak{X}\left(1 - \frac{y^n}{l}\right)^2 \quad . \quad . \quad 2$$

The formula 1 gives the plain parabola $o\ \beta\ s$, Fig. 4, Pl. IV. and formula 2 the parabolic cyma $o\ \theta\ s$, of which the latter indicates the proper progress of the ordinate cross-section of the displacement of a vessel, and agrees with vessels constructed for speed of the present day, while old vessels come nearer to the plain parabola $o\ \beta\ s$, as constructed by Chapman.

I have investigated the progression of the cross-sections in a great many vessels, from most parts of the world, as will be seen in a treatise on the Parabolic Construction of Ships now in progress. Many American vessels agree perfectly with formula 2, of which the U. S. frigate *Niagara*, constructed by the late Mr. Steers, is one. The formula 1, which embodies Chapman's method, is therefore not applicable in modern ship-building, which I think, is the reason why the original parabolic system has not been more generally adopted. It is not always necessary to pay the greatest attention to speed, as there are many other circumstances of greater importance, namely, freight, shallow water, location of metacentre and centre of gravity of the vessel; for which it becomes necessary to arrange the parabolic construction of ships, so that it will accommodate itself to all the requirements, as well as to the taste of the ship-builder. This can be accomplished by raising the ordinate β to any arbitrary power, which we will designate with the letter q, and call it the power of the exponent n, when the final formula will appear :

$$\beta = b\left(1 - \frac{y^n}{l^n}\right)^q \quad . \quad . \quad 3$$

This is the general formula for the parabolic construction. The balance of the Lecture has appeared in the *Journal of the Franklin Institute.*

It does not appear that Chapman attempted to form the water-lines and frames of a vessel by the parabolic method. He says the area of the cross-sections can be approximated by a parabola, placing the vertex at the keel, which cannot give a proper shape to the frame. Inasmuch as the displacement of a vessel is the integral of the areas of the water-lines and cross-sections, and that those areas are integrals of the ordinates in the frames and water-lines, they are all convertible into one another by a common formula, which is the formula 3, and which formula embodies Chapman's system completely; simply by placing $q = 1$. But, by so doing, the constructor is restricted to a stiff and obstinate guide, which will not yield to his taste, and we have the result before us: namely, the ship-builder assumes his independence. It would be futile to attempt to introduce a system of constructing ships that would not accommodate itself to the taste of the constructor. By Chapman's system, when the length, breadth, depth, and the displacement are given, then, the sharpness of the vessel is obdurately fixed, while by an arbitrary value on q the sharpness and ease of the lines can vary considerably, and accommodate itself to the taste of the architect.

Suppose the area, length, and breadth of the load-water-line of a vessel are given, which is substantially the same as if the displacement, dead-flat, cross-section, and length were given ; then Chapman's method will produce the fixed line, say *o m m s*, fig. 5, Pl. IV, while the formula 3 will produce any variety of lines, as *o o o s*, or *o e e s*, or if we wish to go to the extreme the wrong way, we can produce the line *o n n s ;* in fact, the formula 3 can manipulate the displacement the same as one can work a lump of soft clay in his hands. What more is wanted? This is a property of my parabolic system which does not appear to have been appreciated by my correspondents on the subject; but when once fairly laid before them, I am assured they will be the first to appreciate and acknowledge its utility.

This formula 3 is not limited only to ship-building ; but is applicable to a great variety of physical laws. It embraces all properties of heat, such as the pressure and temperature of steam, the law of dilatation, total and specific heat in matter. I prefer to use this formula for the rude and empirical mode of interpolation, and most invariably succeed in bringing it in.

Artificial Light and Lighting Materials. By B. H. Paul, Esq.

From the Journal of the Society of Arts, No. 593.

(Continued from page 210.)

Discussion.—Dr. Marcet, F.R.S., understood Mr. Paul as directing attention to the heating action produced by various lighting agents. This property, no doubt, varied very much in the different agents used —some gave out more heat than others, and some evolved more carbonic acid gas than others. Coal gas produced less carbonic acid than petroleum and other substances of that kind. This subject was important in connexion with the effect of artificial light upon health. No doubt in rooms where lights were burnt for a considerable time, unless proper regard were paid to ventilation, a large amount of noxious gases were evolved. These gases consisted of carbonic acid, sulphurous acid, and others of a more or less pernicious nature. In some cases in rooms where lights were burnt in which the combustion was not complete, there was a formation of carbonic oxide which was most poisonous. When explosions took place in mines not only carbonic acid was formed, but also carbonic oxide, and these were both destructive of life in a very short time, but each separately had a different action in the destruction of life. Carbonic acid gas destroyed life comparatively slowly, while carbonic oxide immediately struck down any one who breathed it. In the case of the lamentable explosion at the Hartley colliery, which was attended with so large a loss of life, he had no doubt that a mixture of carbonic acid and carbonic oxide was formed, and he hoped, for the sake of the victims themselves, that the latter predominated, inasmuch as their sufferings would be less protracted than under the influence of carbonic acid alone. The action of these gases produced such different appearances after death that it could be ascertained by which of them the death had been caused. Carbonic acid gas rendered the blood left in the veins of a dark color, while in cases of death from carbonic oxide the red color of the blood was heightened. The fact of these gases being generated by the combustion of the agents used for lighting purposes made it necessary that the utmost attention should be paid to ventilation. He thought the plan of lighting adopted in the room in which they were assembled was so perfect that it was impossible there could be any accumulation of noxious gases, while the light itself, by being concentrated into one large focus, was of a very efficient character, and ventilation was greatly promoted. The subject of lighting by petroleum having been alluded to by Mr. Paul, he could have wished that some further information had been given with regard to the Rangoon petroleum. From that material had been manufactured a very perfect lighting agent under the title of Belmontine, but he was afraid the supply of that petroleum, as indeed of many other kinds, was considerably on the decrease, but he had no doubt that other sources of supply would be obtained as the original ones failed.

Dr. Bachhoffner said on a recent occasion when they were favored with a paper on an analogous subject to this, he expressed a somewhat strong opinion with reference to the use of hydro-carbons as lighting agents, which opinion he saw no reason to retract. He still maintained that for the ordinary oils of commerce a sufficient guarantee should be given as to their non-inflammability at a temperature below a certain fixed standard; otherwise there was danger in their use for domestic purposes. He did not dispute the correctness of Mr. Paul's statement that, if these oils were used under proper precautions, they would be safe enough: but the great difficulty was to ensure those precautions in ordinary households. Most of the lamps at present in use required to be trimmed and lighted in a peculiar way, and with great care, and this could hardly be trusted to servants. He had no objection to offer against the use of these lighting agents, provided a satisfactory guarantee were given by the retailers of the article that it would not explode at a temperature below 100° at least, though he would rather have a margin of 20° or 30° above that. There was at present a certain amount of danger in the use of these hydro-carbon oils. He was aware that the same objections were raised against ordinary gas on its first introduction. He had no doubt they could easily blow up a house with gas if they were so disposed, but this would only result in most cases from great carelessness. The peculiar odor of gas was a great safeguard in its use, for any escape was at once detected. He remembered there was once an attempt made to deprive coal gas of its odor, but he regarded that as the greatest security they could have against accidents, for if they allowed the taps to be turned on they were apprized of the fact through the medium of the olfactory sense. With regard to petroleum, until there was a satisfactory guarantee when hydro-carbon oils were purchased, that they were not explosive at a temperature below something like 130°, he considered they were not safe for general domestic use.

Mr. Robinson, as one who had had some experience in the use of paraffin oil, fully confirmed the views expressed by the last speaker, as to the necessity for a sufficient guarantee being given as to the temperature at which these oils would inflame. Without that guarantee he considered their use objectionable and dangerous. The system under which they were at present sold was extremely unsatisfactory. A short time since he purchased some paraffin oil which was guaranteed not to ignite upon the application of a match to it in a saucer; but upon testing it he found it ignited very readily. He thought it worthy the consideration of this society how far petroleum oils might be employed for the purpose of heating steam boilers, instead of as a lighting agent. He thought the use of paraffin and petroleum oils highly dangerous with the present tall lamps, which were liable to be upset by the smallest accident, when the most serious consequences might ensue. He fully concurred in the views of Dr. Bachhoffner as to the desirability of a more satisfactory guarantee being given of the degree of inflammability of these oils.

Dr. Bachhoffner said petroleum had been used in America to a great extent for generating steam in steam boilers, and a report had been issued showing that it had been attended with certain advantages.

Mr. Wankyn thought too much importance was attached to the comparison of lighting materials as regarded the relative qualities of carbonic acid produced in combustion. Ordinary air contained from three to five parts of carbonic acid gas in 10,000 parts of air. Experiments made upon air taken from the most crowded theatres of London showed it to contain not more than fifty parts of carbonic acid in 10,000. If they considered the immense quantity of air there was in a room, and made a simple calculation, it would appear that no description of lighting material they could have could produce an amount of carbonic acid that was likely to have a serious effect upon health. He believed it had never been established that the difference between 5 parts and 50 parts in 10,000 had an injurious effect upon the health; and the probability was that the injurious effects of close air were due to something else, rather than to the slight additional quantity of carbonic acid gas. He believed the amount of carbonic acid present under such circumstances was practically of no moment whatever. With regard to the danger of explosion, petroleum had only recently come into use and he believed one of the most effectual ways of guarding against this was to use lamps holding only a small portion of oil. To cause an explosion they required air to be mixed with the vapor of petroleum, and it was only when this vapor was present in a large proportion that explosion was possible. When the lamp was full of oil there could be no explosion; the accidents occurred for the most part when the lamp was nearly empty.

Mr. Tegetmeier said, having investigated the circumstances of a great many accidents alleged to have been occasioned by the explosion of paraffin lamps, he had only met with one genuine instance, which occurred at Bethnal Green in the autumn of last year. In that case the lamp was a cheap and badly constructed one; the oil used was inflammable at 110° (disposing of the alleged safety of oil at 100°), and a woman in attempting to put the light out blew down the chimney, which ignited the oil, and the burning liquid was scattered over her, occasioning a considerable amount of injury. With regard to the inflaming point of oil he understood Mr. Paul to put the point of safety at 100°. He (Mr. Tegetmeier) thought the inflaming point should be placed some degrees higher. He had never traced an accident to oil inflaming at over 120°; therefore he agreed with Dr. Bachhoffner that they ought to have a guarantee of safety up to about 130°. As to danger in the use of really good oil, he believed it was practically *nil;* and with oil uninflammable up to about 130°; they might use the lamp in the roughest manner, and even spill the oil, without the slightest danger.

Mr. Paul, in reply upon the discussion, said with reference to the remarks of Dr. Marcet, as to the relative amount of carbonic acid given out by different lighting materials, it was only necessary to say that as the illuminating power of these materials entirely depended upon the amount of carbon they contained, it followed that for a given quantity of light they must yield a given quantity of carbonic acid. In the table given in the paper the various materials mentioned were seen to contain carbon in slightly different proportions, the greatest amount being contained in olefiant gas, which was about 86 per cent., while marsh gas contained about 75 per cent., and the latter had no illuminating power, while the former produced a brilliant light. It was a mistake to suppose that petroleum had an advantage over coal gas in producing less heat: for the production of heat as well as of light depended upon the amount of carbon; the more light, the more heat, and the more carbonic acid they had. The only room for improvement in the case of gas, as far as he could see, was with regard to the diluting agents of olefiant gas; these were of three kinds—carbonic oxide, carburetted hydrogen, and hydrogen. These differed in the amount of heat they produced, and carbonic oxide was objectionable from its poisonous properties. The improvement would be to substitute hydrogen for marsh gas or carbonic oxide; still he repeated, for a given quantity of light they must have a given quantity of carbonic acid and a given quantity of heat. The production of carbonic oxide under the circumstances in which gas, petroleum or candles were burned, seemed to him to be beyond the range of possibility. Carbonic oxide could only be produced by the combustion of carbonaceous substances where there was a deficiency of oxygen. When a light was burnt with insufficient air the consequence was smoke, which preceded the production of carbonic oxide, and gave sufficient warning of the risk. There was a method of lighting which had been tried, which he had no doubt would eventually be successful, and would be largely used, in which the light was obtained without any carbonic acid being produced. He referred to the lime light, in which the solid substance, instead of being carbon, was a piece of lime, intensely heated by the flame of oxygen and hydrogen. The possibility of using that plan of lighting for ordinary purposes rested upon one circumstance, viz; the production of cheap oxygen. If this could be cheaply supplied there would be no further difficulty about the lime light: but under present circumstances the improvement that could be made in the existing methods of lighting consisted in better modes of ventilation, such as were adopted in this room—a principle first introduced by Faraday, and one which might be regarded as perfect in all respects. With regard to the Rangoon petroleum, the history of it was very simple. It was an extremely good material for the manufacture of hydrocarbon oils, the best of the kind being that known as the Belmontine, of Price's Candle Co., but the cost of Rangoon petroleum in this country was from £20 to £25 per ton, while the American was supplied at from £12 to £18. It did not therefore require many words to say why Rangoon petroleum was not used. With regard to American petroleum he was far from enthusiastic about the future of that. He was disposed to think the oil distilled from coal and other similar materials would become the staple material for the production of these illuminating oils. With regard to the alleged danger attending the use of these materials, he must say he could not agree with the apprehensions entertained by gentlemen whose opinions, however, were deserving of great respect. The idea of being limited in the use of these materials by the stupidity of domestics was a thing he did not like to submit to. The probability of a gas tap being left with the gas escaping was quite as great as that of any blunder through which an accident might be produced by the use of petroleum. Of course in the use of all these materials a reasonable and proper amount of care must be exercised, and, as with ordinary gas, they must have proper apparatus for burning these oils, which apparatus must be carefully managed. The fact that these oils could afford light in country places, where gas was not obtainable, at a price of little more than half as much again as gas, was sufficient to prove that they were a considerable boon to the great mass of the people. There was another fact which was not sufficiently attended to—that was the enormous aggregate consumption of these oils. There were several factories which turned out 30,000 gallons of oil per week, and the number of accidents they heard of was infinitely small when compared with the extent to which these materials were used. At the same time it was quite proper that it should be settled by competent authority what should be the standard of safety in these oils. The Act of Parliament, passed some time ago, unfortunately only applied to the storing of petroleum and other inflammable substances, and left out of consideration other important points. The Act merely provided that not more than 40 gallons of oil, lighting at a temperature below 100°, should be stored within a certain distance of a dwelling-house; but less than 40 gallons of such oil, if ignited, might occasion very serious damage. With regard to the temperature of ignition, if it were possible to use oil that would light at 100° with perfect safety, provided they used proper lamps in a proper way, it was desirable that no impediment should be placed in the way of doing so; at all events, the question should be settled, and the absolute degree of safety established. With regard to the use of these materials for fuel, that admitted of very simple comment. He was aware they had been tried in America, but a slight consideration of the composition of petroleum would show that this was not likely to answer commercially. The heating power of petroleum was not more than 1¼ times that of coal, and while the price of petroleum in this country was about £18 per ton, coal was about £1 per ton. From this fact alone they might judge how far the application of petroleum to heating purposes in manufactures was practicable.

Mr. Symons said that the assertion of Mr. Paul—that heat bears a constant proportion to light, did not agree with the experiments of Dr. Frankland. According to the published tables of these experiments, the heat produced did not bear any constant proportion to the amount of light evolved.

The Chairman thought they could not but feel obliged to Mr. Paul for the interesting paper he had brought before them this evening, and also for the further observations he had been kind enough to make on matters arising out of the discussion. The subject of lighting was a very interesting and important one, both as regarded the lighting of towns and the interior of dwellings. Those who had been in any of the towns of the East, which were entirely unlighted at night, except by the small lanterns people carried about with them, would not be surprised at the fact, that when the Turkish Ambassador came to London, he thought the city was illuminated in honor of his arrival, though the only lights used at that period were oil lamps. He might mention, that long after the introduction of gas into Newcastle-upon-Tyne, he saw a house there blown up by gas. The singular fact connected with this occurrence was, that in that house no gas was laid on; but the pipe of the public lighting having burst, the gas found its way into the house, and the servant, going about with a lighted candle, produced the explosion, which was attended with very serious consequences. That accident, at the time, was very likely to lead to a supposition that gas would prove a very dangerous thing, and that accidents of that kind would be of frequent occurrence. Yet it was the only instance of the kind he had ever heard of, and 40 years had passed over without the repetition of such an occurrence, showing how erroneous a view might be entertained as to the question of danger. Reference had been made to the application of these oils to heating purposes. That was a subject well worthy of most careful consideration, because everything that tended to economize coal must tend to the duration of this country as a great and powerful nation. The Chairman concluded by moving a vote of thanks to Mr. Paul.

The vote of thanks having been passed,

Mr. Paul, in reply to what had fallen from Mr. Symons, said he thought Dr. Frankland would be the last person to dissent from the view he had put forward as to the amount of light, and consequently of heat produced, being in direct proportion to the amount of carbon contained in the vapor burnt. As to the reason why the oxyhydrogen blow-pipe did not give light in proportion to the heat, it was simply from the fact that the gases burnt contained no carbon, and yielded no solid product.

Artificial Lighting.

From the Journal of the Society of Arts, No. 594.

The observations I made in the discussion on Mr. Paul's paper, on the 1st inst., were in reply to his statement—"It was a mistake to suppose that petroleum had an advantage over coal gas in producing less heat, for the production of heat as well as of light depended upon the amount of carbon; the more light the more heat;" "for a given quantity of light they must have a given quantity of carbonic acid and a given quantity of heat." There appears to be a misapprehension of the generally received theory of the correlation of physical forces in these assertions; a deffinite quantity of force will no doubt be given out in the formation by combustion of a certain quantity of carbonic acid, but the following table, from Dr. Frankland's lecture at the Royal Institution, in February, 1863, (on which my remarks were founded,) shows that this force may be given out in largely varying proportions of light and heat. In connexion with the subject under discussion, the table may be otherwise interesting to some of your readers who may not have seen it. It shows the amount of carbonic acid and heat generated per hour by various illuminating agents, each giving the light of 20 sperm candles.

	Carbonic acid.	Heat.	Cost.	
			s.	d.
Tallow,	10·1 feet,	100	2	8
Spermaceti,	8·3 "	82	6	8
Wax,	8·3 "	82	7	2½
Paraffin,	6·7 "	66	3	10
Coal gas,	5·0 "	47	0	4¼
Cannel gas,	4·0 "	32	0	3
Paraffin oil,	3·0 "	29	0	5
Rock oil,	3·0 "	29	0	6½

It will thus be seen, at least according to Dr. Frankland's experiments, that for exactly the same amount of light, tallow emits nearly four times as much heat as paraffin oil, and more than twice as much as coal gas.

W. SYMONS.

For the Journal of the Franklin Institute.

Atmospheric Pressure as a Traveling Force, versus *Animal Power and Steam Locomotives.*

Steam is prohibited from driving cars through this populous city. It could not be admitted without a sacrifice of life, hence horses on our street railways. Time is most appreciated in commercial communities. Almost every one entering a car wishes to be at the end of his route in the shortest possible time, but that cannot be attained or even approached in streets so generally thronged and sometimes blocked with carriages. Twelve passengers is the complement of an omnibus, but rail cars are "never full," though jammed inside and out they stop for every applicant and leave him to find standing room where he can. As on some other lines, the Fourth Avenue cars start every two or three minutes. They run from below the City Hall to 32d Street—nearly 2¾ miles. The time given to the conductor is 30 minutes, each way. The actual average is I believe two or three more. "Add to the number of cars" is the natural suggestion and it will have to be done, though it will add to the number of stoppages and dead-locks at crossings. Soothing one sore it will aggravate another. A few *through* lines would do something but where is there room for them. Increased and uninterrupted speed is the only remedy and it is wholly impracticable, and inadmissible if practicable. The business part of the city is so crowded that stoppages and quarrels of drivers are almost constant. The carmen complain through the press and formally to the city authorities of rail tracks which, they say, their carts cannot travel on without injury to their horses. The feeling may be inferred from remarks of the *Tribune* of June 24. As an historical fact the extract is worth preserving.

"There was another hearing of complaints against city railroads yesterday, before the Common Council Committee. The question is easily narrowed down to a simple proposition: Are the streets of New

York for the whole people or for the carmen? If for the people, the carmen—an integral portion of that people—must take their chances with all the others. If for the carmen, the people must be delayed, obstructed, annoyed, run over, as they are now to a certain extent, and permit their business to suffer for the accommodation of but a mere fraction of their own number. Carmen complain that rails break their carts; then let them keep off the track—they cannot get more than one wheel on at any rate. There is room enough in almost any street to keep clear of the rails. If they have not a load to deliver in a street having rails, there is no good reason why they should prefer that street; if they have business there, they need not drive on the rails; if they necessarily back up across the track, the cars must wait, and the railroad men do not complain, however passengers may grumble if they will insist upon driving on or across the track out of pure spite, as they very often do, to our certain knowledge, they are properly ordered off by the police. We do not say that car-drivers and conductors are models of patience, but we know that it is the especial delight of a great many carmen, stage-drivers, grocery men, fish and vegetable peddlers, and all other divisions of the carrying trade, to delay, thwart, annoy and insult the men who manage cars—-we know that some of them deliberately seek occasions to be annoying, and have often suffered until almost out of patience with their bullying."

Looking beyond the present hour we apprehend a system will have to be adopted that will neither require horses nor interfere with street travel. For want of room on the surface the lines will have to be subterranean or aerial—under or over the houses. At present, decided objections would most likely be urged against both. But, passing that, suppose it were possible to inaugurate a system by which a Fourth Avenue car-load of passengers could be sent through in three minutes, that would be equal to ten of the present trips, and if double the number of passengers were taken it would be equivalent to twenty. That is, work would be done in 30 minutes which now requires 600. In other words 200 trips would be made in ten hours instead of 20: and with a single car instead of a dozen.

Now, as a general proposition, this is both possible and practicable. Still, not to draw too largely at once on public credence, let those who think it too utopian, double the time and say six minutes a trip instead of thirty—less than a mile in two minutes. To some minds even that may seem, if not visionary, in advance of the age and of the arts; but, in reality, it is in advance of neither. Enterprise and intelligence can realize it at once. If too late, or too early, to adopt it here it may not be elsewhere. The main point has already been established. The principle is that of the abandoned *Pneumatic Railways*, with this difference that passengers are to be carried through the tunnel or tube instead of over it. Don't be alarmed. When duly considered, objections will be found more imaginary than real.

"Why, must not blackness of darkness pervade the interior?" Not necessarily, since gas or oil lamps may be carried along as in night cars. But the air in crowded cars is offensive in hot weather; must it not in such tunnels resemble that in the black-hole of Calcutta? No, not in the longest line. No space could be more thoroughly ventilated since fresh air would be constantly streaming in, besides sweeping through at every stopping place. But as passengers are always moving with the piston away from the mouth, how is fresh air to reach them and displace that which enveloped them at starting? Easily; the windage of the piston would, or a valve could, be made to accomplish that perfectly. No air unfit to breathe could accumulate. Then, where is the difficulty of having sun-light by day as well as lamps and candles by night. As the tunnels require no connexion with exterior mechanism, instead of being buried in the ground they may be laid upon it (and where required supported above it,) by the sides of common roads, over fields, or through forests, as circumstances may suggest, and to have plates of glass in the roof and sides. Thick slabs of that material are now used on store floors to transmit light below. I see nothing impossible in the idea that atmospheric tunnels may ultimately be formed chiefly if not wholly of glass instead of iron, romantic as it may now appear.

There are no more limits to the length of atmospheric lines than to railways. Landing and receiving passengers at intermediate stations present but little more obstructions in one case than in the other. At each station a hinged iron door fitted air-tight to its frame (cast on one side of the tube) gives entrance and egress. But does not that involve the loss of the existing vacuum in the untraveled portion? Not at all. The piston must pass the door before it can be opened or the passengers reach it. It has then only to be anchored; and freed as soon as the change of passengers is made. The officer at the station may then either close the door or leave it open till the return trip begins. It would require no fastening inside or out.

The rate of travel will of course depend on the extent or degree of the vacuum, and that on the power of the exhausting apparatus. In the Pneumatic Railway from London to Croydon, about nine miles, the pressure was eight pounds to the square inch and the velocity 30 miles an hour with a train of sixty tons. What would it have been with ten tons and a tube without a slit through its entire length? While some birds fly 60 miles an hour and some, as frigate birds, at a higher rate against the air, there is little risk in saying that passengers may be blown by it through vacuum tubes at a greater speed, and with emotions as bland as those which an agreeable breeze imparts ; or with feelings so slightly unpleasant as to border on pleasure.

A Pneumatic Despatch Company has been recently established in London and mail-bags are being sent through air-tight tunnels. Occasionally a couple of attendants have passed through "without the slightest discomfort." A London Journal says, "the next step of the company will be to lay tubes connecting the markets with the Cambden Goods Station, with a tube to the General Post-Office and Pickford's depot in Gresham street, and these operations will eventually tend to *revolutionize the carrying system of the metropolis, and relieve the crowded state of our principal thoroughfares.*"

Apply the principle to passenger traffic and how much greater the results.

As the connexion between morals and mechanism is inseparable, Humanity has something to say on the subject; and though self-interest may be deaf to its teachings when they conflict with gain, there is no better listener when they harmonize with increased profits. There are two safe criteria for judging of inventions. Every good one is a contributor to morals, direct or indirect: a device that lessens the demand on living force is an example of the former, and that which saves time, of the latter. When air-lines become common we shall cease to witness the noblest of quadrupeds prematurely worn out and dropping from exhaustion in the streets.

It may be presumptuous to augur that steam locomotives and their trains will ere long fail to meet the demands of society; but it would be much more so to conclude that they comprise the perfection of mechanical resource, and are to be the *ne plus ultra* of land traveling for all time to come. In nature nothing is born or matured at once, and in art nothing springs complete from human brains. Inventions, like plants, are designed to grow till they are ripe, and, when all is got out of them that can be, to give place to others. When one has fulfilled its promise in the front ranks of civilization it is to fall back till it reaches the hindermost. Ages may elapse before this is done, or can be done while barbarism endures. Flint implements continued in use through ages of bronze, and iron has not yet reached all savage tribes.

That the rapid transit of men and merchandise is an important, and destined to become a leading element in civilization, no one can doubt who observes its influence on the social, political, commercial, and intellectual world. If it stand still, progress in other departments must be arrested. Whatever may be the extreme limits to speed, there is little risk in asserting that the present mean rate will in time be deemed intolerably slow. It is not every one that is now satisfied with it, and the number is not small who wish it were doubled.

When faith in progress becomes general, improvements in the arts will become common. The more there is of it the less of that depressing unbelief which meets new projects, sometimes with derision, always with suspicion, and against which valuable novelties have to struggle into life. Preferring to rest satisfied with things as they are, it is doubtless ready to ask what possibility there is of any marked advance in traveling by steam, and where the necessity for it? The very objections to mail coaches in the last century, which were to pass over roads good and bad, night and day, at the incredible average rate of ten miles an hour: to the first steamboats also, which if they did not threaten to upset and break the limbs of their passengers, would subject them to the double risk of being scalded to death or blown piecemeal into the air.

Had the leap from ten miles by horses, to fifty by steam, been attempted at once, it would have staggered the boldest, and probably have brought public execration on the proposer; but nothing of the kind can take place. By the law of progress, improvements are gradual —never precipitate.

The timid would not willingly be now among the first to travel at the rate of 80 or 100 miles an hour, if they even had little fear of its taking away their breath. The feeling is a natural one and therefore to be respected, although it has no rational foundation. It is chiefly ascribable to ignorance of the fact, that the highest speed no more affects our bodily organs than the lowest.

Passengers in the cabin of a ship flying before the wind, have no more sensation of going forward than when she is lying at anchor. A balloon rushing upward, or in a lateral direction, appears to the aeronauts stationary and motionless. In night rail trains we walk to and fro, sit and sleep, unconscious of progression as in a parlor or bed-room. In day-time it is the same if we close our eyes to objects outside. Jolts from obstructions and irregularities of roads, with changes of direction and diversities of speed, tell us we are moving. In a perfect system of travel there could be no sense of motion at all, whether the rate was one mile an hour or a hundred, or five hundred. And even that is a snails pace when we look, and we ought often to look, beyond our petty doings to those of the Great Engineer. Our earth is one of a line of passenger cars that conveys us through space at a mean velocity of 68,000 miles an hour, without disturbing a loose brick on a chimney, or displacing a grain of dust, proclaiming the fact that uniformity of motion is equivalent to rest, and suggesting that as with time and eternity, heat and cold, light and darkness, attraction and repulsion,* &c., rest and motion are one. Hence if the time should ever come when 1000 miles an hour can be done, old ladies and young children will be no more inconvenienced than now, and in all probability not near as much.

But what are the main defects, or deficiencies, of the railway system, which make their removal or mitigation already a desideratum? 1. By a certain law the speed is confined within limits which cannot suffice to meet the requisitions of a progressive world, and, 2. It is not at all commensurate with the power expended. A train of cars meets with a resistance from the air that increases with the speed. What it is may be deduced from the force of wind against bodies at rest. When blowing at the rate of 20 miles an hour the force is nearly 2 lbs. on the square foot, at 25 miles over 3 lbs., at 35 miles 6 lbs., at 50 miles 12 lbs., at 100 miles nearly 50 lbs., &c. The cost of power for thus merely clearing the way for a train is therefore very great; but there is an outlay still greater which yields no return, and in itself sufficiently demonstrates that mechanical science is at an incalculable distance from its progressive terminus. Every observer knows how large a portion of the strength of draft animals is expended not on the loads but on the vehicles and drivers. In the weight of carts, drays, and omnibuses, the largest portion of the driving force is unproductive. In steam trains it is believed that with every ton of passengers not less than two tons, some say three, of wood and iron, fuel and attendants, &c., are borne along. That is to say, *two-thirds of the power* is *con-*

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* See Cursory Thoughts on Natural Phenomena and the Unity of Force. New York, 1863.

sumed to make one-third productive, and less than one-third when light loads are carried. Though steam cars are not dragged hither and thither to collect loads, there is always a waste of power before starting and after stopping.

The wear and tear of locomotive engines is great and the cost of working them enormous. Their liability to be thrown off the tracks, and to collisions is constantly enforced on us by current accounts of such costly accidents.

The advantages sought for in the atmospheric system are therefore, greater speed and less outlay of working power—no "rolling stock."

The leading requisition is rapid transit, and for all practical purposes the velocity of a piston rushing into a vacuum is unlimited. At all events, something approaching to 100 miles an hour may, we presume, be attained with equal safety and a smoother motion than by open rail cars. Jolting or jarring would be reduced to a minimum. The passage through the tubes might be on runners instead of wheels, At first nervous persons would be haunted with fear of traveling in what they would call a close tube. But would it be a close one? Rather consider it a vaulted passage with sky lights and windows, and (besides openings to get in and out at stopping places), whose back door remains open till you reach the front one. Granting that it may not be to every one's taste, what is there that is? It certainly offers some advantages over railways which travelers know how to appreciate; as freedom from rain and sleet in winter, wind and dust in summer, and smoke (no trifle) all the year round. Add security against running off tracks and against those terrible collisions so awfully destructive of life and property that continue to occur, and the most timid will be ready to say, "better a close passage free of these than an open one exposed to them.

The speed may be increased from the slowest movement at starting, or be uniform throughout by exhausting the air before starting. In the latter case, there would be a saving of time and power, for the moment one load of passengers arrived, the force to send back another would be accumulating. That is, while a locomotive engine or horses are waiting till a train is ready to move, the power has done most of its work, and has only to finish it by drawing out the remaining air as the piston moves on.

In the simplicity of its mechanism and saving of power the atmospheric system is one of great promise. The power is expended more on the load and less on the vehicle than in any other. There are no massive locomotives and tenders to generate and carry the force to and fro, and in so doing unprofitably consume most of it. On the contrary the motive engines being stationary not a fraction can well be thrown away on the exhausting apparatus, whether a load is in transit or not. Then as there is no resistance in front of a load of passengers the speed increases directly with the force. There is another advantage that deserves more than common attention. In the act of withdrawing the resisting air in front of a load passing through the tube, Nature promptly and without cost furnishes the propelling power in the rear; whereas steam locomotives *gain* nothing by overcoming the opposing fluid. The difference is quite as much as between leading a willing animal after you and forcing a resisting one before you: the more you hurry it the more obstinate and refractory it becomes.

These remarks are thrown out with the hope that they may lead those interested in contemplated new Lines of Railroads to inform themselves of the capacities of the atmospheric system. It is unnecessary to enter into details since most of them have been anticipated. The abortive Pneumatic Railways established the principle completely, and what if it failed in one form of application is that a sufficient reason in this day of great things not to try it in another? If projects not half so full of promise as it is, had always been so treated, the world would not be what it is. Thus tamely to abandon a world-wide desideratum on account of a mechanical difficulty, and not perceiving the lesson that difficulty taught, will never be sanctioned by those who believe Atmospheric pressure is destined to take its place among popular motive powers, and therefore fitted to do what others cannot do, or in all respects do as well. Every force can do something better than another, and it is the business of the engineer to find out what that is.

No mechanical experiment is without good results, negative or positive; and the one referred to, in showing that the load must be passed through the tube instead of over it, will repay to society more than all it cost: not yet perhaps in Europe if the testimony of a recent writer is reliable, viz: that it is there generally conceded that atmospheric pressure is of no use to the business of railways except as a substitute for ropes to pull trains up grades too steep for locomotives. Had a moiety of the money and ingenuity laid out on the attempts to connect the piston with the trains by a factitious, and practically impossible, device been applied to illuminate and otherwise fit up the tunnels for the conveyance of passengers, that opinion had not been hazarded nor endorsed.

The data for determining the comparative economy of the two systems must be wide of the view we take of them, if the expense of working atmospheric be not *one-third* less than that of locomotive lines. The first cost may be greatly in favor of the latter and yet the difference may not be so great as imagined. The durability for centuries of a line of tubes must be taken into the account and the less amount of grading required. Ascents and descents on medium undulating ground would require little or none. It is not however what a line costs but what it earns, and the amount to be deducted from its earnings for working it. That is the main thing to be considered, and it is that which seems to render it all but certain that the atmospheric system is destined to accompany if not to supplant the other.

I am aware that the development and progress of the arts are as much under law as natural productions; that generic inventions appear in a regular sequence, that they can no more come out of their order than living genera, that before society is prepared to receive them they would be deemed worthless abortions of which nothing could be made; and that if it were possible to learn what discoveries in, and applications of, science are to characterize remote cycles of time, they could in all probability be little better appreciated by us than leading achievements of our day could have been ten centuries ago. The future is not to be anticipated in its products any more than in itself, in its philosophy than in its philosophers, in its inventions than in its inventors. All therefore that we have to do, or can do, is to extend and improve the applications of known principles and forces, and in doing that we open the way for and hasten the advent of others.

Although things below the mental are as invisible as those beneath the material horizon, fresh additions to the arts may generally be found by marking the signs of the times. There are always some ready to be adopted. As respects improvements in travel, only one has risen fairly into sight—the atmospheric system. There may be others more promising, open to clairvoyant engineers but we should rather deem them such as are casting their shadows before them, and not likely to emerge into full view for some time to come.

That such will come is certain. Indications of progress everywhere abound, and as respects engineering, and the arts generally, in a quarter the most significant. Of the *metals*, iron has been and is justly esteemed the most valuable and indispensable. The prime agent it is also the measure or standard of civilization. People rise in the scale in proportion as they employ it. This, its destined influence, is denoted by the fact that it occurs more abundantly than most others, and more than all or nearly all of those in common use put together. Its pre-eminence was early recognised and is now more fully conceded than ever. Without it barbarism and semi-barbarism would still have the world to themselves. Without it where had been steam engines, rail roads, gas, and water works, telegraphs and other ancient and modern achievements. They could have had no existence but in the imagination and possibly not even in that.

But passing what it has done and is yet to do, is its glory to be eclipsed? Or are others to be subject to it and their applications to depend upon it for ever? If yea, human advancement is certainly limited; and the end may be thought to be almost in sight, since the proporties of this metal are pretty thoroughly known and to a considerable extent made the most of. Its consumption may be vastly increased but progress implies more than mere consumption. As it interposes serious checks to the extension of engineering, as in beams that bend and chains that break with their own weight, is it to be inferred that the application of metal to these and kindred purposes has reached the climax? Decidedly not.

It is now ascertained that metals exist in greater profusion than iron and with properties that will make them surpass it both as regards the magnitude and variety of its applications. Two, of three most abundant, are *aluminium* and *magnesium*, the former three times and the latter four times lighter than iron, and rivaling in their alloys steel in hardness and tenacity. One is partially and the other wholly free from the cancer to which iron is subject—oxidation from exposure to the atmosphere. These are the very qualities wanted. What could not now be accomplished even in rapid traveling if locomotive engines and their trains were reduced to one-third of their present weight, and two thousand ton steamers to five hundred tons. What heavy and weak affairs our chain cables, suspension and other iron bridges, &c., will one day appear in contrast with the neat, light, and lasting fabrics then in use. But everything—every metal—in its own order.

E.

New York, June 29th, 1864.

On Fibres for Brushes.

From the London Mechanics' Magazine, July, 1864.

There is a great demand just now among brush-makers for some strong supple fibre which may supplement the present supply of piassaba or *bass* received from Brazil. Two strong fibres, the produce of different palms, have been for some time received from the two ports of Para and Bahia. Of late less care has been given to the selection and preparation of these fibres in South America, and they are much mixed with waste and useless fibre. The piassaba fibre, and the so-called Mexican grass, the produce of the leaves of Agate, have quite revolutionized the brush trade by cheapening the cost and replacing bristles. Owing to the improvements in Russia, arising from attention to bacon and ham curing, bristles are less stout and plentiful than they used to be from the wild hogs. The attention of residents in tropical regions may therefore be drawn with advantage to the demand for new fibres for brush-making, street sweeping machines, and chimney cleaning brushes. The midribs of the leaves of many of the palms, a stronger and stouter substance than the kittool and ejoo fibres of Ceylon and the East, are what is wanted, and probably some wiry grasses like the esparto might be experimentalized upon with advantage. They would realize a good price if suitable and to be obtained with regularity and in quantity.

Deposit in Boilers.

From the London Mechanics' Magazine, July, 1864.

Sir:—As you requested me to inform you how I got on with my experiments on removing the deposit from steam boilers with ordinary steam, I beg to inform you of the result.

I had the boilers blown off on Saturday evening, and on Sunday morning at six o'clock I commenced letting the steam into them. I kept the supply of steam up to eleven o'clock, with a slight flame from faggots both in tube and flue; I then opened the manholes. The men went in on Monday, when they succeeded in removing about one-third more deposit than had ever been got out before, although the boilers were working a week less than their usual time. I also find we are doing the same amount of work since with a slight decrease in our consumption of fuel.

By taking these things into consideration, I am inclined to believe that we have done some good, although upon going into the boilers I could not see much difference in their condition. Still it is my intention to try the process every time we clean out, which is about once a month, and I am sanguine that eventually the boilers will become thoroughly clean.

It is my intention to use the soda ash, as you suggested. I should be very much obliged if you would inform me what quantity I ought to use. In my former letter I gave you the dimensions of the boilers. Our consumption of coal is about 1 ton 10 cwt. per day.

Process for making Shot.

From the London Mechanics' Magazine, July, 1864.

Captain Palliser, of the 18th hussars, has produced strong and exceedingly cheap shot by a simple process. Instead of casting the shot in sand and allowing the surface to cool gradually, the metal is poured into a cold iron mould so as to cool the surface with the utmost possible rapidity long before the interior has hardened. In this way a ball is turned out which, to judge from recent trials, combines almost the hardness of steel with the destructive effects of a segment shell.

For the Journal of the Franklin Institute.

Description of Messrs. Borie & Mackie's Cork Cutting Machine.

Patented September 29th, 1863.

On reference to the accompanying cuts, C, C, C, C, are parallel cross pieces of a frame work in front of which revolves the vertical continuous knife, A, the latter being secured to the edge of a horizontal disk, B. In the rear ends of the cross pieces turns a shaft E, to which are secured arms F, the latter being connected by rods *e* to a frame D, which slides in slots *a* in the cross pieces C; the journals of the shafts *c* and *c′* turning in the said frame. On each of the shafts *c* is a toothed pulley J, which engages with a smaller pulley J′ on the shaft *c′*.

To an arm *d*, projecting from the side of one of the cross pieces C, is hinged one end of a plate H, the other end of which is attached to a rubber spring *o*, the latter tending to draw the end of the plate towards the cross piece; and in the said plate is a slot *f* enlarged at each end, through which projects the outer end of one of the shafts *c′*, there being a collar *i* on the said shaft, of such a size that it can pass freely through the enlarged ends of the slot, but not through the smaller portion of the same.

A spring *m*, coiled round the shaft *c′*, is attached at one end to the frame D, and at the other to a collar *n*, which turns freely but cannot move laterally on the shaft.

In suitable bearings at the under side of the cross pieces C turn the journals of two driving pulleys I, which are so situated that when the parts of the machine are in the position shown in Figs. 1 and 3, the peripheries of each of the driving pulleys will be in contact with that of the pully J above it.

When the machine has to be used, a rotary motion in the direction of the arrow is imparted to the knife A and to the driving pulleys I; a block of cork is then inserted, in the manner described hereafter, between the disks, *k k* and a vibrating motion is imparted to the shaft E. The first movement of the shaft E and its arms F in the direction of the arrows (Fig. 1) will move forward the frame D and the shafts *c* and *c′*, the peripheries of the pulleys J will be brought in contact with those of the revolving pulleys I, and a rapid rotary motion will thus be imparted to the shafts *c* and *c′*, and to the block of cork, which, as it is brought in contact with the knife A, is rapidly cut to a cylindrical form.

As the frame D approaches the limit of its forward motion, the collar *i* is brought opposite the enlarged portion of the slot *f*, and the plate H is carried over the said collar, and brought against the side of the cross piece C, by the action of the rubber spring *o*. As the frame is drawn back the collar *i* will bear on the outer side of the inclined plate H and the shaft *c′* will thus be moved laterally so that the disk *k* will cease to bear on the block of cork, which will fall into a suitable receptacle.

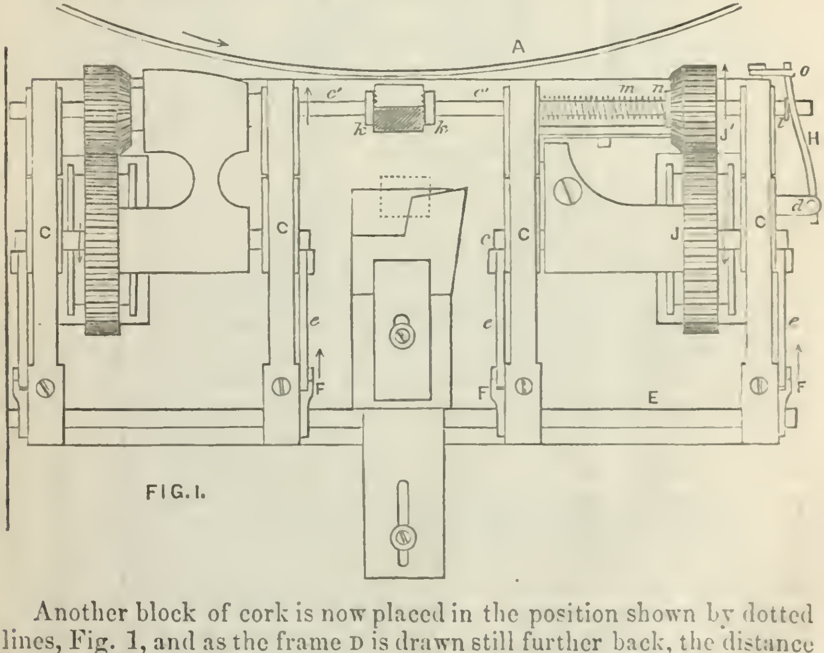


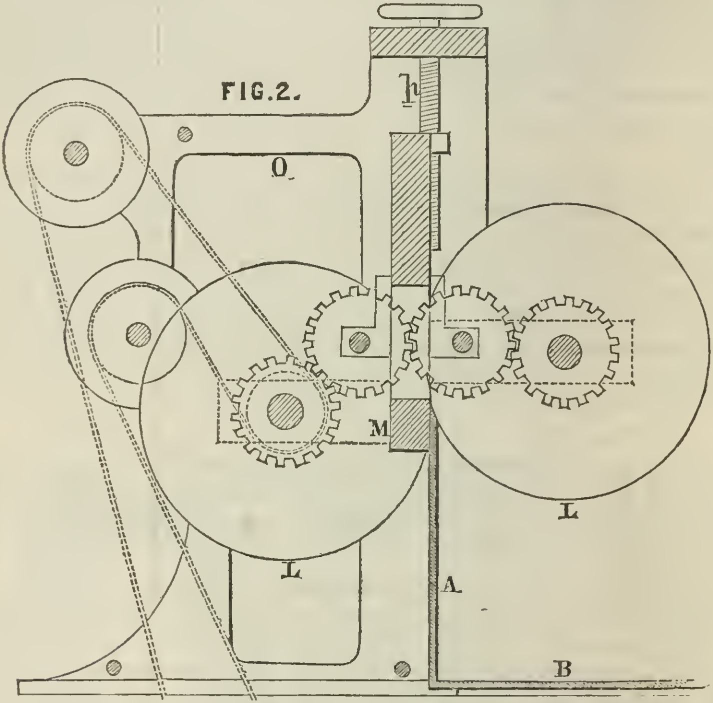
FIG. I.

Another block of cork is now placed in the position shown by dotted lines, Fig. 1, and as the frame D is drawn still further back, the distance between the disks *k* is increased until each is opposite one end of the cork, when the collar *i* will reach the enlarged end of the plate H and will pass through the same as the spring *m* draws back the shaft *i*; the disk *k* on the end of the shaft is thus brought to bear on the cork, the latter being held firmly between the two disks and carried forward to be operated on by the wheel A in a manner similar to that already described.

When desirable, several of these feeding devices may be placed around one cutting wheel A.

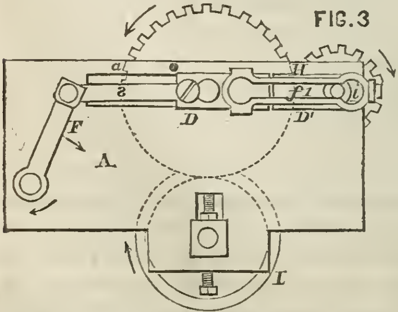
In Fig. 2 is illustrated a device for maintaining the cutting edge of the knife A, perfectly sharp.

FIG. 2.



O, is a stationary frame which is situated at one side of the knife A, and in which slides the frame M, the latter being raised or lowered by turning the screw *p*. On each side of the frame M are brackets in which turn the journals of a grinding wheel L, one of the latter being so situated as to grind the inner edge of the knife to the required bevel while the other bears lightly on the outer edge of the same. The grinding wheels are geared together, and are connected with any suitable operating part of the machine so as to be kept in constant rotation. As the edge of the knife is ground away, the grinding wheels are maintained in contact with the same by lowering the frame M.

FIG. 3



Messrs. Borie & Mackie have now in operation a machine in which there are three feeders and a sharpening device, situated at equal distances around one cutting wheel. An attendant is stationed at each feeder which will feed over eleven thousand corks per day of ten hours, in which time, therefore, 33,000 corks are cut by one machine with three feeders, all of the corks being as smooth and regular as those cut by hand. By setting the feeding device at an angle to the knife, the corks may be cut of any desired taper.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, September 15*th*, 1864.

William Sellers, President, in the chair.

John H. Cooper, Recording Secretary, pro tem.

The minutes of the last meeting were read and approved.

The Board of Managers and Standing Committees reported their minutes.

Mr. Edward Brown, exhibited and described a model of a new manner of working the Slide Valves of Steam Engines.

Mr. Thomas Shaw described a new method of preventing the priming of Steam Boilers.

Mr. Lorenzo Vance exhibited a model of the apparatus for dressing Ship Planks, patented by him, which he described, and explained its advantages.

Professor A. L. Fleury read the following paper which he illustrated by diagrams.

The subject I have the pleasure to explain this evening is one that has recently occupied many of the best European metallurgists, and is one of particular interest to this great iron-producing State.

The want of a uniform and rational system of blast furnaces has long been felt, and judging from the reported results that have been obtained in the use of a new "Universal Blast and Smelting Furnace," the invention of the Russian Mining Engineer, and Major General Woldemar Rachette, this want seems to be fully supplied.

After three years of most perfect success in Russia, where a number of large furnaces have been built at the Iron and Copper Smelting Works of Prince Demidoff at Nischne Tagilsk, at the foot of the Ural Mountains, under the superintendence of the Mining Engineer. Mr. Charles Aubel, these results became known in Germany, and Mr. Aubel then undertook the practical introduction of this fully tested invention in that country. A furnace was at once constructed under his superintendence at Mulheim in Saxony, and the results which crowned this undertaking relieved every doubt as to the working of poor iron ores and the general advantages that have been claimed by the inventor, who has not, as is generally the case with important discoveries, been led to adopt at once his new system; it has taken him many years of costly experiments with various forms, such as annular, elliptical, trapezoidal and horse-shoe-shaped furnaces to develope this last successful modification. Models of his system of smelting furnaces for working iron, copper, lead, and silver-ores, together with the products and the authenticated statements of the advantages that were obtained in Russia over the furnaces of the old system were sent to the great World's Fair at London, and the inventor rewarded with the prize-medal of that department. Letters Patent for this useful invention were granted in Russia, England, France, Belgium, Sweden, Austria, and recently also in the United States.

To give to the members of the Institute a full explanation of this invention is the object of my present communication. This system of blast and smelting furnaces differs from that now in use in several distinct, and very important features, and in order to give a correct idea, I deem it necessary to enter into a more detailed description.

The inventor has called this furnace "*Universal*," because the experience of several years has shown him that with but a slight modification of the general system it can with equal success be adopted for smelting of iron, copper, nickel, tin, zinc, lead, silver, gold, and other ores or mats, also for the direct production of cast steel, and the liquefaction of metals in general.

The *hearth* of the Rachette furnace (as seen in the ground plan of the annexed sketch taken from the working drawings of the last built iron blast furnace at Mulheim), presents the shape of an *elongated rectangle*, and rises up in the form of an inverted square pyramid.

The *shape* of the furnace, *long*, *narrow*, and comparatively *low*, is further distinguished by being *wider* at the tunnel head (the top of the furnace), and having a double series of tuyeres, (blast tubes,) arranged in alternate, interchanging order.

The iron blast furnace has *two working sides* or tymps, and under its hearth and all through the walls *a system of channels and flues*, by means of which the furnace, *while being built, is dried out and heated from the outside to the inside*, and afterwards *ventilated*, in order to preserve the walls from being too much expanded and injured by the accumulating heat.

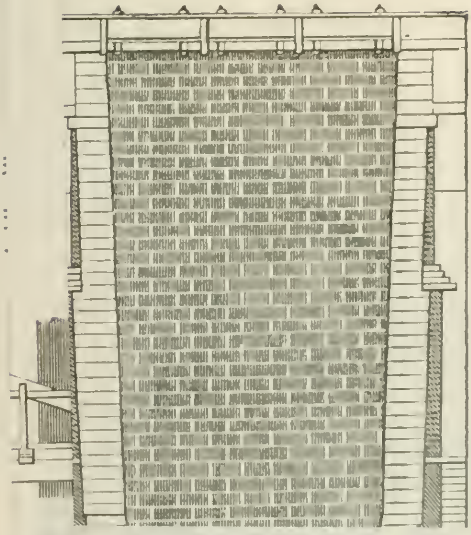
The advantages developed and obtained by these radical features of the Rachette furnace, compared with the results of the present furnace system as stated in the European journals may be summed up as follows:

I. *A great saving* (in some instances *of more than one-half*) *in material, time, and labor in their construction.*

Mr. Aubel states that the cost of a Rachette furnace (of 2600 cubic feet area) at Nischne Tagilsk in Russia, including the tunnel bridge and furnace-mantle, was only 3500 silver Roubles (75 c. in gold each). and estimates that a similar furnace, producing about 20 tons of cast iron per day costs in Germany not more than 10,000 *Prussian thalers*.

At the present high prices of labor and material in this country, I think that a Rachette furnace of 20 tons per day capacity (exclusive of blast, and other machinery), can be built for from 15 to $25,000.

The last constructed furnace at Mulheim, Germany, producing from



third [illegible]
furnace from the so called scaffolding, ([illegible]

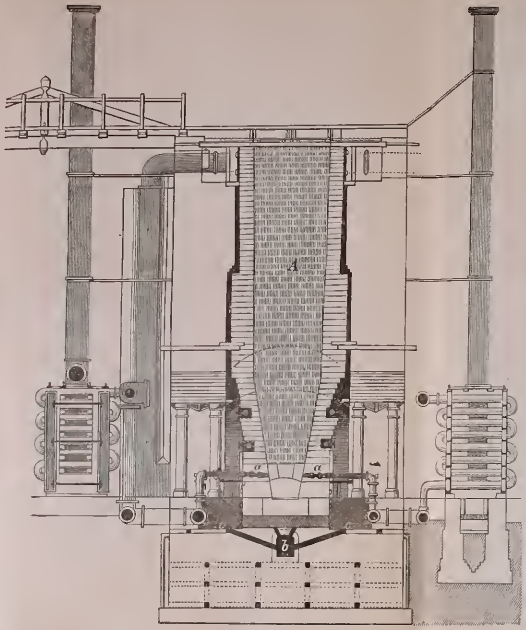


FIG. 2.

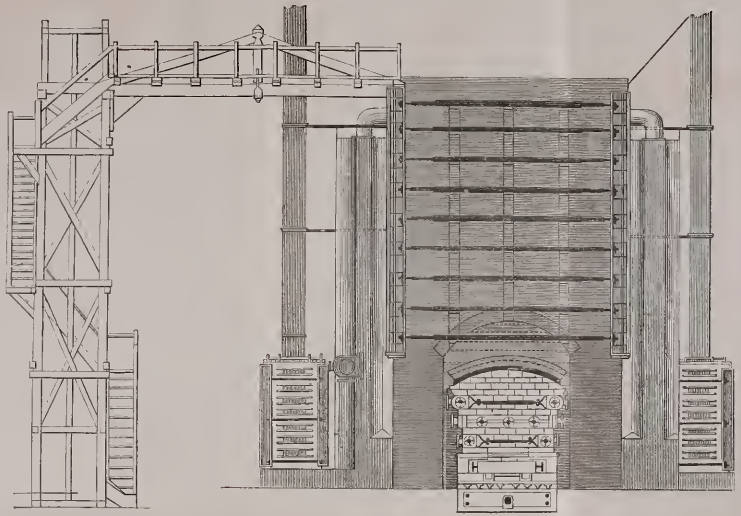


FIG. 1.

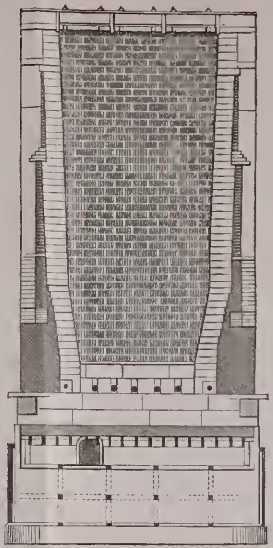


FIG. 3.

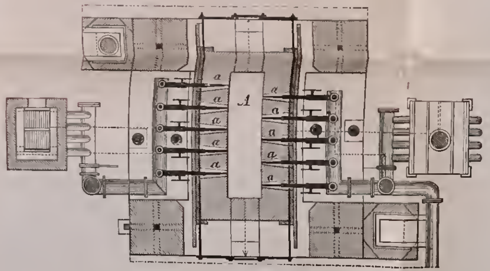


FIG. 4.

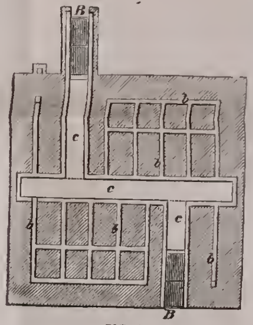


FIG. 5.

20 to 25 tons of cast iron per day, was built *in four months*, and *at once* set to work.

From these data the above stated advantage is at once apparent.

II. *A greatly increased productiveness*, amounting in some instances *to more* than *three times* the old furnaces, *and a saving in fuel and blast.*

An iron furnace of the old round form at Nischne Tagilsk of 5907 *cubic feet capacity*, 51 feet high, worked on magnetic ore containing 67·5 per cent of metallic iron with 4578 cubic feet of charcoal gave 22 *tons of gray pig iron per* 24 *hours.*

A Rachette furnace of 2125 cubic feet capacity, *only* $22\frac{2}{3}$ *feet high* with 12 tuyeres, worked with a consumption of 5341 cubic feet of charcoal the same ore to an average yield of 30 *tons* per day.

The Mulheim Rachette furnace of about 2800 cubic feet capacity yielded on the *thirteenth* day of its working *over* 21 *tons* of the best gray pig iron, and this too *with poor ore*, and with a consumption of $1\frac{1}{2}$ ton of coke to 1 ton of iron. Moreover the ores were such that from 50 to 55 tons of limestone flux had to be used per 100 tons of ore.—With respect to the saving of fuel in this furnace it must also be stated that it only requires from $1\frac{1}{4}$ to 2 *pounds pressure*, while the furnaces of the ordinary form require a blast of double and three times that pressure. Hence at the new Mulheim furnace *half the fuel* formerly required for generating steam for the blast engines *is now saved.* These results are mostly due to the peculiar structure of the furnace.

Being *low*, and possessing *an extended and well regulated* smelting zone, (an advantage that none of the several furnaces in use, round, oval or otherwise shaped can present,) further, being widened at the top, (an advantage Truran, one of our best authorities in iron matters, has fully acknowledged), *the ores only remain seven hours* in the furnace, while the old system of furnaces they remain from 16 to 24 hours.

III. *A better and more uniform quality of pig iron.*

The reported results obtained in Russia and Germany speak unanimously of the good and uniform metal obtained in the Rachette furnace, even as has recently been demonstrated, from an inferior and poor ore.

IV. *An easier management of the process, and less liability to get out of order, while at the same time a better preservation of the furnace is insured.*

The drying and heating arrangement, by means of channels and flues, extending from under the hearth through the walls upwards to the top of the furnace, has been found of great value.

Not only is the furnace thereby at a small cost dried out and heated *from the outside inside*, but it serves also, when the furnace is at work, to ventilate it, and to carry off the excessive radiated heat, and prevents the furnace from becoming overheated. This arrangement fills a third important office, namely that of making it possible to rid the furnace from the so-called scaffolding, (what however no good iron master should allow to take place) I mean thereby the adhering of iron to the sides of the furnace. The flues are so arranged that by closing up certain apertures the heat can be concentrated on any part of the furnace and so the scaffolding melted down without recurring to the expensive blowing-out remedy.

The number and peculiar arrangement of the *tuyeres* deserve also some consideration. They are not placed opposite each other—but in an *interchanging alternate order.* A regular and continuous *smelting zone* is thereby obtained, while at the same time the peculiar rotary motion that is imparted to the smelting particles facilitates their contact with the carbon of the fuel to a much better extent than the oval or circular system. *Five to six tuyeres* have hitherto been considered best adapted for *iron* smelting about 2 feet apart.—For *copper* and *lead* 16 to 24 *tuyeres* have been employed to advantage. The inventor has also successfully used so called "*slit blast boxes*" whereby the wind is introduced in one continuous sheet. The manner of charging the furnace is the same as used in others, except that the ore and flux are fed more around the outer edges of the tunnel head, while the coal is placed more toward the middle. The flux and ore are thereby brought more into immediate contact with the blast and smelting zone, leaving the fuel more toward the middle. This has been found to give a decided improvement in the saving of fuel and more perfect reduction of the ore.

The following dimensions have been found by Mr. Aubel to be the best adapted for *iron furnaces*:—

Height about 30 feet; width of hearth at the base 2¾ to 3 feet; length of hearth 16 to 18 feet; width at the top about 7 feet.

We will now consider the success which has been obtained by the use of the Rachette furnace for the reduction of *copper* and *lead ores.*

In a *copper*-smelting furnace of this construction in Russia 53½ *tons* of copper ore were smelted *in 24 hours*, while in a furnace of the old construction not more than 8½ *tons* could be produced in the same time. A furnace of this kind therefore replaces 6 to 7 furnaces of the old system. Comparative tables, sent from St. Petersburgh show further remarkable economy in fuel (from 25 to 33 per cent.) and a great saving in labor:—

The working of the Rachette furnace has been found to be much easier and the slags more regular and about 0, 1 per cent poorer than those taken from the old furnaces. The duration of a smelting period in Russia is usually 50 to 60 days, the Rachette furnace however, has made a working of more than 140 days without interruption.—As was the case with the iron furnaces, the attention of eminent German metallurgists was also directed to the adaptation of the Rachette furnace for smelting *copper* and *lead.*

Mr. O. Hartman in Saalfield, Germany, communicates some interesting trials which he has made with a diminutive Rachette furnace for smelting of copper. This furnace has a length of 4½ feet below and above, is 10 feet high, 3 feet wide at the hearth, and 4½ feet wide at the top.

These tuyeres on each side supply the blast; the hearth from the fire wall to the front has a fall of 4 inches and the tuyeres are so placed as to correspond with this fall and are 23 inches above the hearth. The blast is supplied by a rotary fan-blower of 3 feet diameter, making about 1100 revolutions per minute. The result is reported to be of a most satisfactory nature. In 24 hours 15 to 20 *tons* of ore were worked up, and the fullest satisfaction in every respect obtained.

Another furnace on the Rachette system has recently been built at the Altenau, (Germany) silver works for smelting of *lead* ores and is now in most successful operation.

This furnace is 20 feet high, 7 feet 4 inches long, 3 feet below and 4¾ feet wide on the top. 5 tuyeres on each side (1½ in. diameter), furnish the blast about 16 inches above the hearth. It was set to work on the 28th of June this year, and has smelted up to the 2d of July, 10½ *tons of lead* with the average consumption of 1 *pound of coke to* 10 *pounds of ore*, while another furnace of the *old* construction *did not* in the same time produce more than 4 *tons and* 150 *tons of lead* with an average consumption of 1 *ton of coke to* 7 *tons of ore.*

Mr. Aubel mentions also the adaptability of the Rachette furnace for the smelting of *cast steel*, either from the ore or from the adapted materials, such as wrought and cast iron, or cast iron and oxides. I beg to call your attention more particularly to this point of importance. The above named gentleman states that he has succeeded in melting *Platinum* before the mouth of a tuyere;—there is therefore no reason why this class of furnace will not answer for the production of large masses of steel.

I learn that the assignee in this country, Mr. Alexander Trippel, chemist at the Lehigh Zinc works at Bethlehem, Pa., has invited Mr. Charles Aubel to come to this country and assist in the introduction of the Rachette furnace. Several parties have already declared their intention to build furnaces on this new system and a smelting work is contemplated in the neighborhood of New York where furnaces for smelting lead and copper on this principle will be erected.

The great importance of this improvement to all iron masters in its economy and results has induced the owner of the patent in this country to make arrangements to form a company of such parties as will at once interest themselves in the introduction of the system.

Those wishing further information can address Mr. A. Trippel, Bethlehem, Pa., A. L. Fleury, care National Hotel, New York, or C. B. Norton, Esq., 151, Broadway, New York.

DESCRIPTION OF PLATE V.

Fig. 1 Front View.
" 2 Vertical Section. *A*, the interior; *a*, *a*, tuyeres; *b*, the chamber; *c*, *c*, flues.
" 3 Longitudinal Section.
" 4 Horizontal Section of the Hearth. *A*, the hearth; *a*, *a*, the tuyeres.
" 5 Horizontal Section of the Hearth. *c*, the heating chamber; *b*, the flues.

A Comparison of some of the Meteorological Phenomena of SEPTEMBER, 1864, *with those of* SEPTEMBER, 1863, *and of the same month for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W from Greenwich. By J. A. KIRKPATRICK, A. M.

	September, 1864.	September, 1863.	September, for 14 years.
Thermometer—Highest—degree, .	81·00°	83·00°	95·00°
“ “ date, .	24th.	7th & 17th.	12th 1851.
“ Warmest day—Mean,	75·00	77·17	85·20
“ “ “ date,	24th.	7th.	6th 1854.
“ Lowest—degree, .	51·00	41·00	39·00
“ “ date, .	26th.	27th.	25th 1856.
“ Coldest day—Mean,	58·50	51·67	51·30
“ “ “ date, .	25th.	26th.	30th 1853.
“ Mean daily oscillation,	12·30	14·65	16·46
“ “ “ range, .	4·82	4·22	4·67
“ Means at 7 A.M., .	63·22	61·57	62·61
“ “ 2 P.M., .	70·78	70·68	74·38
“ “ 9 P.M., .	66·32	64·70	66·54
“ “ for the month,	66·77	65·65	67·84
Barometer—Highest—Inches, .	30·117 in.	30·312 in.	30·430 in.
“ “ date, . .	8th.	23d.	16th 1851.
“ Greatest mean daily press.	30·072	30·244	30·381
“ “ date, . .	7th.	23d.	16th 1851.
“ Lowest—Inches, .	29·450	29·281	29·281
“ “ date, . .	24th.	18th.	18th 1863.
“ Least mean daily press.,	29·533	29·460	29·403
“ “ date, . .	24th.	18th.	16th 1858.
“ Mean daily range, .	0·103	0·116	0·122
“ Means at 7 A.M., .	29·786	29·940	29·957
“ “ 2 P.M., .	29·760	29·896	29·917
“ “ 9 P.M., .	29·795	29·939	29·939
“ “ for the month, .	29·780	29·925	29·938
Force of Vapor—Greatest—Inches,	0·741 in	0·784 in.	0·991 in.
“ “ “ date, .	24th.	17th.	6th 1854.
“ “ Least—Inches, .	·258	·166	·161
“ “ “ date, .	25th.	26th.	29th 1860.
“ “ Means at 7 A.M., .	·442	·423	·468
“ “ “ 2 P.M., .	·439	·452	·489
“ “ “ 9 P.M., .	·454	·449	·507
“ “ “ for the month,	·445	·441	·488
Relative Humidity—Greatest—per ct.,	97·0 per ct.	90·0 per c.	100·0 per ct.
“ “ “ date,	5th.	18th.	2d 1854.
“ “ Least—per ct.,	39·0	38·0	29·0
“ “ “ date, .	16th.	22d & 26th.	2d 1859.
“ “ Means at 7 A.M.,	74·5	72·6	78·1
“ “ “ 2 P.M.,	57·8	57·6	55·9
“ “ “ 9 P.M.,	69·5	69·8	74·0
“ “ “for the month	67·3	66·6	69·3
Clouds—Number of clear days,* .	8	11·	11·2
“ “ cloudy days, ·	22	19·	18·8
“ Means of sky cov'd at 7 A.M.,	62·3 per ct.	54·3 p c.	56·0 per ct.
“ “ “ “ 2 P.M.,	70·7	53·7	52·6
“ “ “ “ 9 P.M.,	47·7	40·7	36·2
“ “ “ for the month,	60·2	49·6	48·2
Rain.—Amount,	7·317 in.	0·978 in.	4·073 in.
No. of days on which Rain fell,	13.	7·	8·1
Prevailing Winds—Times in 1000	S69° 25′W·244	N47°7′ W ·115	S89° 25′W.189

* Sky one-third or less covered at the hours of observation.

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FOR THE

PROMOTION OF THE MECHANIC ARTS.

NOVEMBER, 1864.

CIVIL ENGINEERING.

Steam and Water Pressure Gauges.

From the London Mechanics' Magazine, June, 1864.

A brief review of the different very ingenious and scientific instruments employed for ascertaining fluid pressure may be useful, and the examination will certainly not be without interest. Some of these instruments are based upon very philosophical principles, and it will be worth while to see how these principles withstand the rough contact of daily practice. Steam gauges are now looked upon by no means as "luxuries," though they have been so termed by an eminent writer on the locomotive. Their necessity is recognised, not merely as measures of the safety of the boiler, but also as affording a finger-post to the fireman, who at once knows by the indicator the state of the steam. Partly for this reason, one or two fitters are employed on some of the French lines all round the year solely to travel up and down and keep the steam gauges in order. It is true that a practised touch at the much less delicate safety valve lever will perform a somewhat similar service, but no engineer need be told that a weighted lever is a very rough measure of fluid pressure. And the observation applies to the water pressure of a hydraulic press as well as to steam or air pressure.

Pressure gauges may be divided into four distinct kinds, according to the leading constructive principle adopted :—1. The mercurial pressure gauge—upon the principle of hydrostatic pressure ; 2. The air manometer, in which the principles of both hydrostatic and pneumatic pressures are combined; 3. Gauges in which the pressure of steam is deducted from its temperature; 4. Metallic gauges, in which the elasticity of metal is used as a measure of fluid pressure. This last kind of gauge may be said to have almost entirely superseded the others for most purposes, on account of the very little attention they require, and from other advantages.

The mercurial pressure gauge consists of a U-shaped tube like a syphon barometer. Its lower part contains mercury; to its legs is attached a divided scale, and one leg communicates with the vessel containing the fluid while the other is open to the air. The difference of the two levels thus shows the difference between the pressure of the steam or water, and the pressure of the external air. When an iron tube is used, the level of the mercury has to be shown by a steam peg or float on its surface. The water stand-pipe, used in the old low-pressure engines to feed the water in by hydrostatic pressure may be said to have been a water-pressure gauge. In both cases, an undue excess of pressure led to either the water or the mercury being blown out of the pipe,—rather an expensive kind of safety-valve, in the matter of quicksilver. The common mercurial pressure gauge may be said to be the only gauge which *per se*, ocularly demonstrates its own exactitude. As, however, the column of mercury would require to be some twenty-eight inches high in order to balance the fifteen pounds pressure of the atmosphere, or fifteen pounds pressure of steam, or a head of about thirty feet of water, it is needless to say that such a gauge is inapplicable to a high pressure boiler. Common gauges should, however, be graduated from a standard gauge of this kind. The use of a small safety-valve for the purpose shows great ignorance. To graduate them from a metallic test gauge of course saves much trouble to the maker, but the plan is scarcely as much in the interest of the buyer. Some fifteen or twenty years ago, a complicated variety of this gauge was in use in France, under the name of the *Manomètre à air libre de Richard.* It is fully described in one of the numbers of our contemporary, the "Annales des Mines," for 1845. It consisted of a syphon with several branches, in communication one with another, filled with mercury in the lower part, and with water in the top portions, in such wise that the differences of level of the mercury in the tubes were added to each other. The same French periodical for 1847 contains a description of an ingenious instrument invented by M. Gazy Cazalat. The steam acts on a piston of a certain diameter, the mercury on a larger piston, and the pistons are kept tight by membranes of vulcanized india-rubber. By properly proportioning the relative surfaces of the two pistons, the column of mercury in the open tube may be made as short as one likes. Neither of these ingenious instruments is now in use, having been superseded by the metallic gauge.

The same fate may be said to have attended the air manometer, although an improved form of this instrument has been lately brought forward by Mr. Allan, of Perth, the inventor of the straight link motion. The common air manometer is a long vertical glass tube, closed at the top end, which contains air. The mercury in the bottom part soon gets oxidized, and discolors the glass, so that the level cannot be read. The refraction of the glass also distorts the scale. A slight rise of temperature of the compressed air would also greatly affect the indications. As a consequence of Mariotte's law of the compression of gases, the higher pressures are more difficult to read off in a tube of equal diameter throughout. This objection has been obviated by Mr. Allan and others by means of tapering the tube towards the top, but it does not appear that the other defects have been met at the same time. Steam pressure gauges of this kind are forbidden by law under most of the continental governments.

Thermomanometers, or steam gauges indicating the pressure of steam by means of its temperature, generally consist of a large thermometer graduated to atmospheres. Callardeau's thermomanometer (1829) was of this kind. The fragility of the instrument, and its want of sensibility and slowness of indication, have caused it to be abandoned. We remember to have seen an instrument of this kind in use on a boiler in Mr. W. Fairbairn's works at Manchester, some years ago. Thermometers have been made on the well-known compensating principle used in chronometers and compensation pendulums. Two bent strips of copper and steel were riveted or soldered together, and the unequal expansion by heat of the two metals caused a movement which was registered by a wheel and quadrant in the usual way. We do not know whether steam gauges have been attempted on this plan—but the galvanic corrosion consequent on the contact of the two metals in the presence of moisture would porbably soon alter the instrument.

Under the broad heading of metallic gauges comes a great variety of instruments for measuring fluid pressure by means of the graduated motion of a metallic spring. Mr. D. Gooch's steam gauge may be placed under this classification. It consists of a piston working in a cylinder against a spiral spring—in fact, very much like an ordinary MacNaught indicator without the drum. They have not come into use mainly on account of the incrustation of the boiler clogging the piston. Similar instruments were used on French locomotives nearly twenty years ago. The cylinder was kept pretty clear by being separated from the boiler by means of a tap. But it is plain that the intermittent indications thus afforded were not much more than those to be obtained from the safety-valve lever. Messrs. Easton and Amos, the hydraulic engineers, use a gauge upon this principle on a hydraulic press at their works, but they do not trust to it very much. Messrs. Smith's (of Nottingham) gauges may also come under this category. A volute spring takes the pressure through the intermedium of an india-rubber disk stretched across the tube. Messrs. Salter & Co. also use an india-rubber disk in this way, which performs the same function as in Gazy Cazalat's old gauge. Amongst other objections these gauges are liable to injury in course of time, through the induration of the india-rubber, caused by its partial corbonization. The cheapness of the plan has led to several others of its form; but it is one thing to make a gauge that will be accurate at first, and it is another thing to make one that will keep right for a length of time.

A German writer has neatly classified the different principles of purely metallic gauges under the following heads:—1. Every hollow ball, subjected internally to the pressure of a fluid, will keep its spherical shape. 2. Every hollow elastic vessel, subjected internally to the pressure of a fluid has a tendency to assume the spherical shape. 3. Every hollow cylindrical tube, subjected internally to the pressure of any fluid will strive to keep its cylindrical shape. 4. Every straight tube, the cross-section of which is not circular, and which is subjected laterally to fluid pressure, has the tendency to assume the cylindrical shape. 5. Every hollow bent tube, the cross-section of which is a circle, and which is subjected internally to fluid pressure, will keep its shape. 6. Any bent tube, the section of which is elliptical, and which is subjected internally to fluid pressure, will have the tendency to straighten itself, if the longer axis of the ellipse is at right angles with the plane of curvature of the tube, that is to say, the extremities will have a tendency to increase the distance between each other. The contrary will be the case if the longer axis of the ellipse coincides with the plane of curvature of the tube, and the tube will then have a tendency to curve itself still more—to bring its extremities nearer to each other. Schaeffer's, De Witt's, and the numerous gauges formed out of a number of superposed hemispheres, come under principle No. 2. Schaeffer and Budenberg's gauges are fitted with a corrugated circular steel plate to receive the steam pressure. The plate is coated with silver on the steam side. This is intended to prevent corrosion; but we have sometimes noticed a certain amount of galvanic corrosion from the contact of the two metals. The corrugations are concentric, being similar to the concentric ripples caused on the surface of water through the vertical fall of a stone. The back of the pointer is fitted with a spiral spring, in order to absorb any looseness of the working parts. De Witt's gauges consist of a hollow chamber formed of two disks, each consisting of three or four thin steel plates. The chamber is collapsed by the pressure, and thus affords a longer range than the single disk of Schaeffer. A similar gauge to Schaeffer's was patented in England in 1844 under the name of Fontainemoreau. Cuny, in Germany, made some years ago gauges in which the motion of a metallic plate was taken off by a column of mercury, the rise and fall of which in a tube indicated the variations of pressure. This plan is simple, but the results are liable to be affected by temperature, and other causes. Gauges made of a number of flattened spheres appear difficult to keep from leakage, and no gauge of this kind seems to be commonly used. Under principle No. 6 come Bourdon's admirable gauges patented in France and in England in 1849. The greatest breadth of the tube is generally placed in the direction perpendicular to the plane in which the tube is curved, and the tube then becomes more curved or less curved, respectively, according as the pressure within, is greater than without, or the pressure without is greater than the pressure within. Although the Bourdon gauge has been accused of taking a permanent set, and also of taking what may be termed a temporary set—that is, not coming back at once when the pressure is withdrawn—our own experience leads us to prefer these gauges to any other. The American makers of the Bourdon gauge, Messrs. Ashcroft, of Boston, patented several years ago a form of Bourdon gauge made with two springs or tubes one inside the other. Messrs. Dewrance, in the Borough, have patented a similar plan in England, but they use three brass tubes—drawn by Muntz's process—one within the other. This plan is stated to afford more elasticity within a smaller compass. Messrs. Bailey, Blake, and Bailey, of Manchester, have also lately patented what they probably call an improvement on the Bourdon gauge. The tubes are constructed either in two portions, one side of brass and the other of steel, or by "fixing a steel band against one side of a brass tube." There is no doubt that "by this means they obtain a greater deviation of shape in the tubes when subjected to pressure or acted upon by heat;" but we are inclined to predict that galvanic corrosion will make quick work of the steel half, and thus affect the indications.

As a commentary on the above we may mention that Mr. Amos, C.E., the Consulting Engineer of the Royal Agricultural Society, makes it a practice to test all the steam gauges of the boilers that are intended to work at the Shows. They are all tried in a parallel row on a steam pipe, to which is affixed the standard gauge. We have thus seen almost every one of these gauges give a different indication. Such store does the French government lay on good steam gauges and their proper state, that it is specially decreed that every boiler in France should be fitted with a pipe and flange, to which a test gauge can be at once attached by the government inspector. The obvious deduction from these facts is, that the greatest care and attention should be used in the selection, repair, and watching of the steam gauge. The employer of steam power will then find his reward, not merely in the increased safety of his boiler, but also in the saving of fuel.

Atmospheric Railways.

From the London Mechanics' Magazine, September, 1864.

Exactly fifty-four years ago, a Mr. Medhurst proposed that a brick tunnel should be built and applied to the conveyance of passengers, at speeds never more than dreamt of before. Within the brick tunnel a pair of rails were to be laid, and on these rails a suitable vehicle, very similar in its general arrangements to an ordinary railway carriage, was to travel. The cross section of the brick tube, as proposed, would have been egg-shaped, with the maximum width above. The rails would have rested on projections springing from the side walls near the bottom. To the rear of the carriage, a piston, so to speak, formed of boards suitably framed together, would have been affixed. This piston would have nearly fitted the tunnel. Whether any expedients were proposed by which the space between its edges and the brick-work could be made partially air-tight, we are not prepared to say. It is not likely that a scheme so perfect in principle as this was, would be found wanting in detail. The carriage and piston thus provided, and put in place within the tube, air was to be forced in behind by means of a large pumping apparatus, very similar, we believe, in general design, to the blowing engines at present used at our iron-works. The pressure of the air thus pumped in, would, it was contended, prove sufficient to propel the carriage with its load of passengers at very high speeds. Mr. Medhurst lived before his time. The scheme never got beyond a model for obvious reasons. In the first place, the steam engine was not yet perfected, and the obtention of the necessary motive power for the blowing machinery was by no means easy. In the second place, people had a very great and perhaps natural antipathy to the idea of being placed within a tube, dark and cheerless, and blown to their destination; and thus a really valuable invention fell to the ground. It is easy, however, to see that Medhurst's was no ordinary mind. In this scheme we have the embodiment of nearly all that constitutes the modern railway—the iron rails, the high speeds, the accommodation for passengers, have a great deal in common with the present system of locomotion, and all this, be it observed, was designed twenty years before the Rainhill trials inaugurated the railway system. After Medhurst came Vallance and Pinkus, gentlemen who proposed certain alterations, the principal idea being involved in the reduction of the size of the tube; the alteration of its position with regard to the carriage, by placing it between the rails and below the floor; and the exhaustion of the air from the space in front of the piston, instead of its compression within the space behind; but this last had been already proposed by Medhurst, who seems to have left scarcely a point overlooked. Messrs. Vallance & Pinkus had no better success than Medhurst, and it remained for Messrs. Clegg & Samuda, years afterwards, to develope the system on a practical scale on the London and Croydon, and Dalkey and Kingstown railways. The atmospheric principle as tried on these lines is now well known to be wholly unsuitable to the demands of an extensive traffic, and, as far as the country is concerned, the vacuum tube and the piston carriage have been banished for ever in favor of the locomotive. With the introduction, however, of the underground metropolitan railway system, the old scheme of Medhurst bids fair to be revived. Indeed, there is hardly room to doubt that it is, of all others, the most suitable for the exigencies of this species of traffic. In the pneumatic dispatch we have, on a small scale, all that Medhurst proposed; and there can be no room to doubt, from the success which has already attended upon the labors of the company, known by the same name, that the system can be extended to the conveyance of passengers without any practical difficulty whatever. During the last few months, too, Mr. Rammel, the inventor of the pneumatic dispatch scheme, has been laboring at the Crystal Palace to provide a model line—the first on which regular passengers have been conveyed—which would serve to bring all these advantages fairly before the public.

The tube extends from the Sydenham entrance, to the armory near Penge-gate—a distance of about a quarter of a mile, and it is, in fact, a simple brick tunnel, nine feet high, and eight feet wide—a size that renders it capable of containing an ordinary Great Western Railway carriage. That actually working in the tube at this moment is handsome and commodious. The piston is rendered partially air-tight by the use of a fringe of bristles extending nearly to the brick-work of the tunnel and its floor. A fan 20 feet in diameter is employed to exhaust or to force in air, and perhaps it is impossible to devise any other expedient so well calculated to answer the required purpose. It must be remembered that either a plenum or a vacuum equivalent to ·5 of an inch of mercury is quite sufficient to propel even a heavy train at a high speed on a moderately level line. In the present instance the motive power is supplied by an old locomotive borrowed from one of the railway companies, which is temporarily mounted on brick-work. The tires have been removed from the driving wheels, and these last put the fan in motion by straps.

The line, we have said, is a quarter of a mile long; a very small portion of it, if any, is level, but it has in it a gradient of one in fifteen—an incline which no engineer would construct on an ordinary railway; and as it is not a level line, so it is not a straight one; for it has curves of only eight chains radius, which are shorter than those usually found in existing railways. The entire distance, 600 yards, is traversed in about 50 seconds, with an atmospheric pressure of but 2½ ozs. The motion is of course easy and pleasant, and the ventilation ample, without being in any way excessive. All the mechanical arrengements are so simple and must be so obvious we imagine, that it is needless to dwell on them. We feel tolerably certain that the day is not very distant when metropolitan railway traffic can be conducted on this principle with so much success, as far as popular liking goes, that the locomotive will be unknown on underground lines.

Krupp's Steel Tires.

From the London Mechanics' Magazine, September, 1864.

Messrs. Prosser & Son, the American representatives of the Essen firm, publish a circular containing some interesting facts, of which we select a few. Thus we learn that there are manufactured at Krupp's per day some one hundred tires, of which number one-half are for Great Britain alone. The magnitude of the tire manufacture can be estimated by the fact that forty steam hammers are employed on tires alone at this establishment. Messrs. Prosser & Son state that they have in their possession a turning, taken from a Krupp tire at the Erie Railway shops at Dunkirk, U.S., which measures 727 feet, showing the extraordinary toughness and homogeneousness of the metal.

The circular gives some interesting facts regarding the wear of the tire in America. On the Chicago, Burlington, and Quincy Railway, a goods engine weighing 34 tons ran 29,500 miles, and the wear was only 3-32d of an inch, and the tires are considered good for another year before coming to their first turning; on the Ohio and Mississippi

Railway, a set of these tires on a 32-ton engine ran 60,485 miles with a wear of $\frac{5}{16}$ths of an inch; on the Erie Railway a set of these tires, on a freight engine with four driving wheels having a crushing weight of $6\frac{1}{2}$ tons on each wheel, ran 21,450 miles, with a wear of less than $\frac{1}{16}$th of an inch, and Assistant Superintendent Brooks expresses the opinion that they will run 100,000 miles before coming to their first turning. Several other instances are mentioned of the great wear of the tire in this country. Mr. Prosser remarks: "That there is a great fallacy in comparing the wear of iron with Krupp's cast steel tires on the first turning; for, when an iron tire requires turning the first time, it is usually half worn out, and sometimes even more than that: whereas, Krupp's wear equally well from first to last, however many times they have been turned. This is in consequence of the great resilience of the *peculiar* metal which does not suffer the slightest disintegration under the heaviest load."

Since 1854—55, Mr. Krupp has furnished the Bavarian State Railway with,—

740	tires for driving wheels.
640	tires for tender wheels.
664	tires for trailing wheels.
1766	tires for carriage wheels.

The General Director of this road says:—"The durability and security of the Krupp tire has proved so economical that we have determined to use none other. Of the driving wheel tires, 664 were put in service in 1854; and of these, only 29 had been worn out. Of the carriage wheel tires put in service in 1857—58, not one has worn out or proved defective up to the present time," June, 1862.

A set of Belgian (D'Acier Lamine, puddled or semi-steel) tire, ran but 18,640 kilometres, (11,582 miles,) with a wear of a shade over one-eight of an inch.

With the same wear, a set of Krupp's tires ran 65,774 kilometres, (40,871 miles.)

This comparative test was made on the Luxembourg Railway.

It would appear, from this and other data, that for each quarter of an inch worn these tires will run on good roads some 80,000 miles. $2\frac{1}{4}$ in. tires will usually bear turning three times, wearing as well at last as at first. Their total life or mileage may therefore be stated at 300,000 to 400,000 miles, according to character of service performed.

On the Power required to overcome the Vis Inertiæ of Railway Trains.

From the London Mechanics' Magazine, September, 1864.

Mr. Peter W. Barlow read a paper "On the Power required to overcome the Vis Inertiæ of Railway Trains, with description of machine to propel trains between stations without locomotives," and the following is an abstract:—It was reported to the inventor, who was engineer of the South Western Railway for many years, that when the North Kent Railway was opened to the public, a great consumption of coke arose. He examined into the cause, and found by a series of experiments made on locomotives, and on the atmospheric railway, that the loss arose from the power required to overcome the vis inertiæ of the train. It then became necessary to have locomotives of greater tractive power and weight, which had increased until this day, when they were of sufficient power to put a train of 80 tons into a velocity of 18 or 20 miles per hour before the last carriage left the platform. Having observed this, it occurred to the inventor to try using hydraulic power for the length of the platform, that a degree of momentum might be given to the train which would be sufficient to carry it to the next station, on railways where the stations are frequent, and the power of this operated, as was represented by an attractive force of eight tons, acting over a length of 300 feet, as sufficient to put a train of 60 tons into a velocity of 34 miles an hour and which velocity was sufficient to carry a train a mile and quarter on a level from its own vis inertiæ, and therefore half a mile beyond the next station, if the distance were three-quarters of a mile. He estimated that one 30 horse-power stationary engine, by these means, would propel trains every five minutes a distance of one mile at a cost of £1 per day. He explained that the economy of this arrangement arose partly from using stationary power instead of locomotives, and partly for more rapidly overcoming the inertiæ of the train, which he explained by models he laid before the Section. The inventor's estimate of the length the train would run, with a velocity of 34 miles, was proved from experiments made with that object on the Lancashire and Yorkshire Railway by M. Pambour.

Captain Selwyn said that in America a 15-ton engine had done the work, upon an incline, of 30 tons by adhesion. The invention was one of Mr. Armstrong's, who was well known in connexion with telegraphic science in America. He placed a coil of large copper wire around the driving wheel, below the semi-diameter. It was hung there from the frame of the engine, consequently the driving wheels rotate inside it without touching it at any point. A current was sent from a 12 cell battery of 18 inches in height and 4 in diameter, and the electro-magnetism produced in the coil was sufficient to produce the adhesion to the rail. The wheel never skitted while the electro-magnetism was on, and by preventing this skitting additional power was obtained.

Sir W. Armstrong said it was quite obvious that any principle of this kind, which dispensed with the weight of a locomotive and tender, would lessen the amount of locomotive power to produce the run. If successful, and applied to underground railways, the greatest objection was got rid of to those railways. He had no hesitation in saying that it was perfectly practicable to produce power in the way proposed; but whether the system would have to contend with practical objections and difficulties strictly appertaining to the working of railways, that was a point upon which he could not give an authoritative opinion, for it was a matter more for the consideration of gentlemen who were connected with railways to see whether the working of the system was practicable. When a thing was right in principle, it was quite astonishing how practical difficulties melted away in the course of time and experience. It was likely a great many objections would be urged, and he could conceive many himself, but he would not venture to say that something of the sort would not prove to be practicable. He should be glad to hear what gentlemen conversant with the working of railways would have to say on the subject, and he thought the Section ought to entertain it, and give it a fair and careful consideration.

Mr. Fairbairn said that, in obtaining a momentum of 30 or 40 miles an hour in a space of 100 yards, the question arose whether it would not be dangerous and destructive to the comfort of the passengers in the train. He agreed with Sir William Armstrong in the correctness of the principle, and that the time would arrive when it would be accomplished. In the metropolitan railways it would he very desirable to dispense with the steam engine, and whether it was done by the system recommended by Mr. Barlow, or the system of ropes, the same as the Blackwall railway was worked for a number of years, it would be much more comfortable to passengers than the present system of locomotives. The matter deserved a close investigation, and he hoped that something might come of it.

In answer to a question,

Mr. Barlow said brake power would be applied by the guard.

Mr. Vignolles said he thought that after some practice many other difficulties which appeared at present might be overcome. The question was, whether within a distance of 100 or 150 yards a velocity could be obtained of 40 or 50 miles an hour, for in many cases that velocity would have to be obtained to overcome a rise. That appeared to him to be a considerable difficulty independent of others. It was well known on the Blackwall railway that the distance between the last station and the last but one was effected by casting off the train from the rope, and allowing it to arrive at the station by its own velocity. There were a number of difficulties which must be overcome, not only in experiments, but practically, and the system now proposed was, in fact, almost reverting to the stationary system.

Mr. Barlow said he did not think there would be any jerk in the train in getting up the speed within the distance he had referred to.

The President said the paper had re-opened the inquiry into the relative merits of stationary and locomotive power, which was fully discussed 20 years ago, since the period the question was considered to have been set at rest. It was a loss of power to have to transport the motive power along with a train, the dead weight being 45 tons, and, *a priori*, it was an advantage to be without the dead weight of the locomotive tender; yet for certain particular reasons all engineers had for many years arrived at the conclusion that, with all the disadvantages of the system of working by locomotive engines—tested by any system of fixed power that has yet been devised—it was infinitely the best, so that in all cases where fixed power had been provided at vast expense, that power had been removed and locomotives substituted. There was no doubt that railways in some particular localities were assuming a form which rendered it not inadvisable to re-consider the question, or, in fact, to compel engineers to re-consider it. Rightly or wrongly, the inhabitants of the metropolis were condemned to travel in subterranean ways. For the sake of streets and buildings, and for the sake of avoiding the destruction of property, it seemed to be ordered that the inhabitants of London should not travel in daylight, but underground. It was quite evident that with the growth of the metropolis and increase of traffic in underground railways, the nuisance must become insupportable in consequence of the engines emitting nauseous gases. The question would become one for engineers; and it was therefore we felt much indebted to Mr. Barlow for bringing the subject before the Section. In his opinion, railways were entering on a new phase, and a new motive power could not be arrived at without a great deal of consideration.

Railway Working Expenses.

From the London Artizan, Sept., 1864.

The total working expenses of the railways of England and Wales in 1863 amounted to £ 12,659,618, against £ 12,050,581, in 1862; of the railways of Scotland to £ 1,617,204, against £ 1,520,056, in 1862; and of the railways in Ireland to £ 750,412, against £ 697,772 in 1862. The aggregate of the United Kingdom was thus £15,027,-234 in 1863, against £ 14,268,409 in 1862. The length of the line in operation at the close of 1863 was 12,322 miles, at the close of 1862 11,551 miles. The totals given do not include steamboat, canal, and harbor expenses; and the figures for 1863 are also exclusive of the working charges of the Oswestry and Newtown, Cowes and Newport, Brecon and Merthyr Tydfil Junction, Cork and Kinsale Junction, Dowlais and Hereford Hay, and Brecon. The ratio of expenses to receipts appears to have been reduced last year to 48 per cent., against 49 per cent. in 1862. The working expenses of last year may be analyzed as follows:—Maintenance of way and works, 18·95 per cent., against 18·99 per cent. in 1862; locomotive power, 27·62 per cent., against 27·79 per cent. in 1862; repairs and renewals of carriages and wagons, 9·33 per cent., against 8·71 per cent. in 1862; traffic charges (coaching and merchandise), 27·92 per cent., against 27·95 per cent. in 1862; rates and taxes, 4·20 per cent., against 4·18 per cent. in 1862; Government duty, 2·63 per cent., against 2·63 per cent. in 1862; compensation for personal injury, &c., 1·19 per cent., against 1·11 per cent. in 1862; compensation for damage and loss of goods 0·46 per cent., against 0·48 per cent. in 1862; legal and parliamentary expenses, 1·30 per cent., against 1·54 per cent. in 1862; and miscellaneous, 6·40 per cent., against 6·62 per cent. in 1862.

Railway Accidents.

From the London Artizan. Sept., 1864.

In the year 1861, 79 passengers were killed and 789 injured by railway accidents in the United Kingdom; in the year 1862 on an increased number of lines 35 passengers were killed and 536 injured;

and in the year 1863 on a still increasing length of lines 35 passengers were killed and 401 injured. The number of passengers in 1863 was 204,635,075, without including 64,391 season and periodical ticket-holders. Estimating even that these last traveled on an average only 100 times each, the number of passengers killed in 1863 was less than one in 6,000,000, and of passengers injured less than one in 500,000. Of every five passengers killed, three lost their lives through their own misconduct or want of caution, so that the number of passengers killed from causes beyond their own control was less than one passenger in 15,000,000. Of the passengers killed last year 12 met their deaths by getting out of, or attempting to get into trains when in motion, 5 by incautiously crossing or standing on the line at a station, one by leaning out of the carriage window on approaching a bridge, (since widened), one by getting out on the wrong side of a carriage, one (in Ireland) by getting upon the roof of a carriage and walking along the train. Of the 13 passengers killed in 1863 from accidents to trains, 3 lost there lives through collisions between trains and ten from trains getting off the line, 7 of the 10 in the accident on the Hunstanton line caused by a heifer being on the rails. Of the whole number of accidents to passenger trains in the United Kingdom reported to the Board of Trade in 1863—52 in all, exactly one a week and precisely the same number as was reported to the Board in 1862—32 were caused by collisions with other trains, 10 by the trains getting off the rails, 6 by their running off the proper line through the points being wrong, and only 4 from anything breaking or getting out of order.

Improvements in Metallic Piers and Piles.

From the London Artizan, Sept., 1864.

Mr. J. Phillips has just patented some improvements in piles or cylinders for piers, embankments, coffer-dams, and other structures that are wholly or partially under water. The invention relates to the means of rendering more or less perfectly water-tight the vertical joints, interstices, or spaces existing between the metal piles or cylinders employed for structures that are wholly or partially under water, and consists in forming grooves, recesses, shoulders, or flanges upon the contiguous sides of such piles, and then inserting into such grooves or recesses, or against such shoulders or flanges, pieces of wood or other suitable material, so formed that the pressure of the water causes them to fit tightly against one or more surfaces of the grooves, recesses, shoulders, or flanges of two contiguous piles or cylinders, thereby preventing the water from penetrating through the joints, interstices, or spaces between the two piles to the back of the same. It is preferred to make the grooves in the piles, and the pieces of wood to be inserted therein, of a rectangular form, and to form the lower extremity of the piece of wood with a double incline, so that when driven into the ground it is made of itself to press against the back surface of the two grooves and the side surface of one of them. In some cases the grooves and pieces, of wood are formed so that when the latter are in their places, wedges or packing pieces may be inserted so as to press the piece of wood against the back surfaces, or back and side surfaces of the grooves independently of the action of the water, or the grooves or pieces of wood may themselves be made of inclined surfaces so as to produce a wedging action. In some cases the pieces of wood are compressed before inserting them into the grooves, so that when they come in contact with the water they swell out, and thus of themselves exert a pressure against the surface of the grooves. India-rubber, felt, or cork, in combination with iron, may be used in place of wood for forming water-tight joints in the said structures.

The Great Explosion of Gunpowder; and the Breach in the River Embankment.

From the London Builder, No. 1131.

Warnings of danger to this metropolis from the storage of large quantities of gunpowder, even at places so distant as Purfleet, Plumstead, or Belvedere, have been occasionally uttered,—though they have not received the same attention as those referring to the accumulation of petroleum and other combustibles, in buildings amongst the streets of London. But the magnitude of the danger has been demonstrated since our last number appeared; and consequences hitherto unapprehended, involving, from the particular explosion, the inundation of a large area of valuable reclaimed land, and that of the extensive low-level inhabited districts in the south or south-east of London itself, besides injury and delay to the sewerage works, have been averted only through a most fortuitous conjuncture of circumstances, the presence of mind and the skill of one or two individuals, and the immediate energy and laborious exertions of a great force of men.

On Saturday morning last, at about twenty minutes before seven o'clock, there occurred at a certain spot on the southern bank of the Thames, near Belvedere, and distant about one mile above the church of Erith, and the same distance below the reservoir and Outfall of the whole main drainage of that side of the river, an explosion of a quantity of gunpowder which appears to have far exceeded in the amount that of any explosion in time of war, whilst the sound and concussion reached to places at a distance of eighty, ninety, or approaching one hundred miles. The quantity is estimated to have been 1040 barrels of 100 lbs each, or in all nearly 46½ tons. It was contained in two magazines, which stood close behind the embankment or river-wall of the Thames, and in two barges outside; but the quantity that was in one of the buildings formed a considerable portion of the whole, and amounted to 750 barrels. The destruction of life, or personal injuries, by the explosion itself, fell almost exclusively upon the few individuals, and their families, who were in charge of the magazines, or on board the barges, or engaged in the work of loading or unloading; and it may not have exceeded about twenty individuals five of whom are dead whilst we write, and five are "missing" or represented by fragments of heads, arms, feet, or mere pieces of flesh. Individuals not connected with the magazine, nor on board the barges, however, were killed or injured; amongst the killed were two men who were at the time about proceeding to their work in certain repairs to the river-wall at a place adjacent to the scene of the explosion. The destruction of property however, was immense; though it is rather to be realized from the accounts, albeit inadequate, than discovered by a visit to any single spot. Where stood the buildings, there are now scarcely ruins, and the bricks of the magazines strew the ground as so many pieces cleaned of their mortar, better than could have been done by hand. A wide cavity in the earth shows where stood the principle building, a substantial structure, belonging to Messrs. John Hall & Son, of the Faversham Mills, and about 50 feet square. It was erected five or six years ago, at a cost of about 3000*l*. The other magazine, variously stated as about 60 or 70 yards distant, or as about 300 or 400 yards, belonged jointly to the Elterwater and the Lowood Gunpowder Companies, and measured about 40 feet by 30 feet. Each building was in two floors, and had a wooden jetty for the unloading and loading of vessels. The other buildings were three cottages, occupied by the two store-keepers, one of whom is living, and by the work-people, and were at some distance from the magazines. The barges were blown into fragments, which fell into the river, or with some portions of the buildings and their furniture, and of human limbs, were projected to incredible distances. Some of the business papers were carried into Woolwich Arsenal, distant four miles. This latter place was, by the inhabitants of Woolwich, at first thought to be that of the explosion. The embankment wall, for about 300 feet in length, was forced into the river; and the marshes of Erith and Plumstead, and parts of London itself, which the water covered before the period of authentic history, were laid open to the rising tide. The consequences of this portion of the calamity, to which we have alluded, have at present been averted, as we have said; but in their full extent, they are to be estimated only by professional readers, acquainted with the districts in and east of London, and their levels, with the formation of the banks of the Thames, and with the accounts of some previous breaches. Now, the public mind rests impressed chiefly with the recollection of the concussion, and with the facts including those of the injury done in the villages of Belvedere, Erith, and Plumstead, in the town of Woolwich, and in parts of the eastern and southern districts of the metropolis proper. Glass was broken, and window frames were forced in; so that in Belvedere, distant about three quarters of a mile from the magazines, and in other places, houses have been rendered uninhabitable. Ceilings and chimney pieces were broken, and walls were so shaken, that at least one house, and this one at a considerable distance, has since fallen. Even in London, in the Minories and Aldgate, plate glass was broken; whilst at the Crystal Palace a door was burst open; and in Norwood, a house that was in erection has been greatly injured. Such effects as these last were experienced at distances of 10 or 12 miles. Though three distinct shocks are reported to have been felt, the almost instantaneous spread of combustion from grain to grain of the powder, barrel to barrel, floor to floor, and building to building, is surprising, even with the knowledge of the properties of gunpowder, and the infinitesimal thread of a train, or leakage, that suffices to establish a connexion between one enveloped quantity and another. The occurrence will doubtless receive attention from men of science; but we may note that one statement refers to the falling down of grains of powder apparently unconsumed.

It is now seriously discussed, whether the storage of large quantities of gunpowder in places distant but one mile, or even ten miles, from a populated district, should be permitted. Half-way between the site of the catastrophe, and that of the Southern Outfall of the sewerage, is another magazine, to which happily no matter in combustion gained excess, though glass was broken. Tiles were blown from roofs of buildings further distant. Close adjoining the property of the Metropolitan Board of Works, is yet another magazine, which was empty, but to which the attention of the Board is, we believe pointedly directed. Further west, or above Crossness Point, is still another magazine, this last belonging to Government.

The chimney and buildings of the Outfall works, received no damage beyond that of the fracture of plate glass of the value of about 150*l.* But had the marshes been inundated, the Outfall sewer itself, with its numerous ventilating shafts, as well as side entrances,—and without considering the existing state of the several channels in connexion with the reservoir,—would have been flooded; and the sewage would have been dammed up, even as far as Battersea. The invert of the sewer at the outfall, is 28 ft. 6 ins. below the high-water level; whilst the surface of the ground where the engine-house and other buildings stand, is itself 6 or 7 feet below the level. A breach in the embankment, beyond the jurisdiction, or works, of the Metropolitan Board, is thus seen to be a serious matter of consideration. Fortunately, on this occasion, there was a body of workmen who were able to proceed at once to the scene of the disaster.

Every circumstance of time and tide, availability of competent direction, presence of men at the works of the Outfall, vicinity of a great and disciplined military force with tools and appliances, and even the soil at the spot, happened to be favorable; and a more serious disaster than has occurred within recollection, by a breach in an embankment of the Thames, was by the greatest exertions staved off, and at length was, as we have said, averted. The manner in which the whole business was conducted, merits record in these pages; and the instance may be instructively compared with the experience of previous accidents as related in various books, where the loss to landowners, and the injury to the navigation of the Thames, through the years duration of breaches and by the repeated failures of attempts to close them, is graphically related.

The great destruction of property, and probably of life, and the loss and delay which we have endeavored to convey an idea of, would have ensued from the breach at Belvedere, but for the circumstances to which we have alluded, and the course immediately taken. When the explosion occurred, it was the most fortunate circumstance that the tide was low; but there were only about four hours wanting to the time of high-water. The explosion had forced the greater portion of the materials of the embankment, in the 300 ft. of length acted upon, into the river, only throwing up on the foreshore a low mound, not capable of affording any protection. In the 300 ft. gap which was formed, the space which had been occupied by the foundation or base of the embankment wall was filled with broken and shapeless masses of earth or clay, which had formed part either of the consolidated mass of wall, or of the site of the magazines. The few men who were engaged on another portion of the embankment, were, at the moment, the only hands available for work obviously necessary for the exclusion of the rising tide, but which to be finished, not in the most substantial way, in three or four hours time, demanded the labor of many hundred men.

Amongst the residents of Erith, who were all roused by the explosion, was Mr. Lewis G. Moore, an engineer who has been connected with Mr. Furness, in the contrivance of some of the means of executing the work of the Thames Embankment of these present days. Led to the spot, he immediately saw and appreciated the impending calamity, and recognised the necessity of setting to work a much larger force than that of the few men who were at hand. He at once despatched his card, with a huriedly written request, to the resident engineer, of the Main drainage works, or his representative, to bring immediately all the men, with barrows, picks, spades, and other requisite tools, that he could muster.

The explosion had startled every one at Plumstead. Mr. F. R. Houghton, one of the engineering staff of the Metropolitan Board, and representing Mr. Grant and Mr. Bazalgette, is resident there; and he was aroused, and was soon on his way. He was met by Mr. Moore's messenger. The men at the Outfall works seem to have had the idea at once, that their services would be required; and they started, about 400 in number, on the moment that Mr. Houghton gave the order. To both Mr. Moore and Mr. Houghton the highest praise that we could give them would not be too much; and the first of these gentlemen has most deservedly received the thanks of a public meeting at Erith. Whilst Mr. Houghton, with Mr. Webster's navvies were on the way, and immediately upon their arrival, Mr. Moore directed the filling-in of the interstices of the lumps of earth, with puddle, rather as the best extemporized foundation, than as a sufficient substitute for a properly consolidated one. The work was punned in and rammed as well as time would permit; but it became evident almost immediately to Mr. Moore and Mr. Houghton, that the force under them would be insufficient. They then forwarded a communication to Major-general Warde, the commandant at Woolwich; and the result showed that "the system" must have undergone a change calculated to gratify the people of England; for, by half-past nine o'clock, detachments of sappers and artillery, to the number of about 1500, were on the ground, with appliances of military engineering. They were under the immediate command of Colonel Hawkins, of the engineers; but General Warde was also present. They were followed by the 5th Fusiliers, and the Marines, some of the force being necessary to keep the ground; for, as the day advanced, great crowds were attracted from the surrounding places and from London. A considerable amount of work had been done before the troops arrived; but it was necessarily hurried, as it was essential to keep above the rising tide. The work contended with the tide, to use the words of those who were present, "inch by inch;" but it was kept always above the tide.

The troops brought with them a few thousand bags, such as, filled with sand, are used in the construction of sand-bag batteries; these were filled with clay, which was fortunately to be had by digging; and the bags were passed from hand to hand, and laid in the form of an arch or plan, to make the landward-side of the upper half of the embankment, puddle being filled in between them, and the whole being rammed down, now in a more complete manner than had been possible at first. At length, about two or three o'clock, the work was completed, having withstood the tide. In the night of Saturday, it sank, as had been expected, 6 or 8 ft., and water trickled through. The military and navvies were actively occupied, backing up and ramming the work, on Sunday, when the embankment was exposed to the waves washed by a heavy gale, and stood the test. Next day, we believe, it passed into the hands of the Dartford Commissioners, when a band of navvies were engaged, still further consolidating; and the work was reported as secure as the original wall. On Wednesday, however, there were still 200 navvies engaged; and it had been deemed prudent to fill up the hole in the foreshore, caused by the explosion of the barges, with chalk stone, of which 100 tons were thrown in. At one time, on the Saturday, the danger seemed so imminent, that Mr. Moore proposed, if he did not actually make arrangements for, the scuttling and sinking of a dozen barges loaded with clay, to form a breakwater in front of the breach; but this arrangement, if Perry's reasoning on his experience at Dagenham be correct, could have been only a forlorn hope: as there would have been scour under the bottom of the barges; and, in two or three tides, a deep channel might have been scooped out.

The manner in which the military went to their work, elicited the admiration of all who saw it, including that of the navvies; and the civil engineers named, have expressed their warm acknowledgments. It is evident that without the force and dicipline of the great body of men brought upon the ground on Saturday, and again on Sunday, the previous labor (not bating a particle of the credit due for it) would have failed to cope with the difficulty.

How the matter of accounts with Mr. Webster, is to be settled, we have not heard; but there seems some doubt whether actions will lie against the proprietors of the magazines, by any of the parties who have suffered, or whether the insurance offices can be expected to treat the injuries to property insured only against fire as having resulted therefrom; though one or two offices are understood to be ready so to consider the injuries.

We have hinted that there is some reason to apprehend that the embankments of the Thames, which are said to make in the total 300 miles, are not as they should be for security. The danger averted,—and, as we have shown, through a most fortuitous conjuncture of circumstances,—will, we hope, incite the several Commissioners, and the Conservators of the Thames, to consider whether better survey, and more frequent reparation of the embankments and sluices are desirable; and whether it would not be well to prevent such risk as is proved to be incurred by the storage of gunpowder in a position where explosion may cause a breach.

For the Journal of the Franklin Institute.

General Problem of Trussed Girders. By DE VOLSON WOOD, Prof. of C. E., University of Michigan.

(Continued from page 236.)

If the diagonal bars act as ties, and *b c*, Fig. 5 is subjected to a strain, we will make a section just at the left of *b*. We might use the former section and take the origin at *c*; or we might let the origin remain at *a*, but in the latter case we would have two moments, one the moment of *b c*, the other of *c d*. The first being the simplest, we will take the origin at *b*.

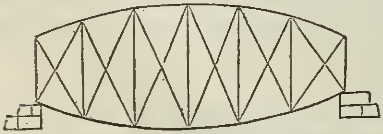
Considering as before the forces at the right of the section, and we have to consider the strains on the infinitely short parts of the bars *a b*, *b c*, and *c d*. To produce tension on this part of *b c* the force acts from *b* towards *c*. Therefore we have $a_2 = 90° + \theta$ and the other angles as before. These in (10) give

$$\left.\begin{aligned} \mathrm{F}\cos i - \mathrm{F}_1\cos i_1 - \mathrm{F}_2\sin\theta &= 0 \\ \mathrm{F}\sin i - \mathrm{F}_1\sin i_1 + \mathrm{F}_2\cos\theta &= \mathrm{V} - \Sigma_0^{x_1}\,\mathrm{P} \\ \mathrm{F}_1\,h\cos i_1 \qquad &= \mathrm{V}x_1 - \Sigma_0^{x}\,\mathrm{P}x \end{aligned}\right\} \quad . \quad . \quad (12)$$

Equations (12) may be deduced from (10) by substituting minus θ for θ. Equations (10) or (12) are applicable to Fig. 4 by observing the signs of the angles. It must be observed that at the left of the middle the signs of i and i_1 change and become negative.

9°. *Let the lower chord be convex downwards*, as in Fig. 6. Equations (10) are immediately applicable by observing that at the right of the middle, i is positive, i_1 negative and θ positive or negative according to the inclination of the bar; while at the left of the middle, i is negative and i_1 positive.

FIG 6



Example. Let the quantities be as in the preceding example, only observe that the inclination of the lower chord is—20°, and we find from (10) that for a load over the whole length;

$$\mathrm{F} = 218,\ \mathrm{F}_1 = 143, \text{ and } \mathrm{F}_2 = -107.$$

The negative value of F_2 indicates that it is a tie.

10°. The same equations apply to the case where the upper chord is convex downwards and the lower convex upward as in Fig. 7, by observing the signs of the angles.

Fig 7

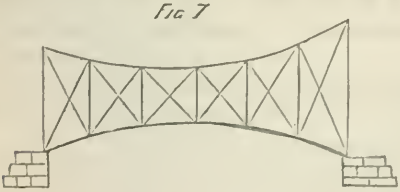
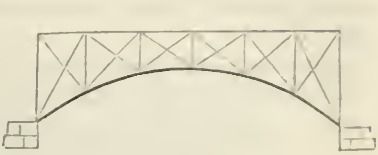


Fig 8

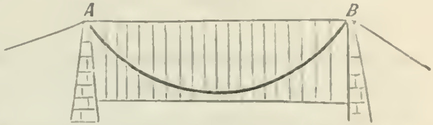


11°. Let the upper chord be horizontal and the lower one convex upwards as in Fig. 8.

Make $i = 0$ in (10) and call the horizontal force in the upper chord H and (10) becomes

$$\left.\begin{array}{l} F_1 \cos i_1 + F_2 \sin \theta = H \\ F_1 \sin i_1 + F_2 \cos \theta = V - \Sigma_0^{x_1} P \\ F_1 h \cos i_1 \qquad V x_1 - \Sigma_0^{x} P x \end{array}\right\} \quad . \qquad . \qquad (13)$$

Fig 9



At the left of the middle, i_1 is negative.

12°. Let the lower chord be convex downwards.

In this case we have only to change the sign of i_1 in (13). If we suppose that all the ties and braces are omitted—or what is equivalent, suppose $\theta = 0$—we shall have the analytical condition of the suspension bridge. Making i_1 negative and $\theta = 0$ in (13) and we have

$$\left.\begin{array}{l} H = F_1 \cos i_1 = 0 \\ \quad F_1 \sin i_1 + F_2 = V - \Sigma_0^{x_1} P. \\ \quad F_1 h \cos i_1 \; = V x_1 - \Sigma_0^{x} P x \end{array}\right\} \quad . \qquad . \qquad (14)$$

In the suspension bridge the land cables resist the strain which is now thrown upon the chord AB, Fig. 9. From the first of these we observe that for $i_1 = 0$; $H = F_1$ *i.e.* at the lowest point the tension of the cable equals the compression of the upper chord. Also from the first $F_1 \cos i_1 = H$, that is, the horizontal component of the tension at any point equals the tension at the lowest point, and this is true whether the curve be parabolic, catenarian, or any other form.

From the second of (14) we have

$$F_1 \sin i_1 = V - \Sigma_0^{x_1} P - F_2$$

in which F_2 may be included in $\Sigma_0^{x_1} P$, and thus not appear as a separate term. Hence we have

$$F_1 \sin i_1 = V - \Sigma_0^{x_1} P \quad . \qquad . \qquad (15)$$

which shows that the vertical force resolved by the arch (or cable) at any point equals the portion of the load which is supported by the pier generally diminished by all the load between it and the same pier; or equal the load between the lowest point of the curve and the point considered.

If the load and ties be considered continuous, and the loading follow any algebraic law, the cable will be a continuous curve, and the preceding equations may be more conveniently expressed in the following way:

Let $f\ (x\ y)$ be the equation of the curve,

s any portion of the arc,

$\text{T} = \text{F}_1 =$ tension at any point

then $\cos i_1 = \dfrac{dx}{ds}$ $\sin i_1 = \dfrac{dy}{ds}$

Hence, (14) and (15) become

$$\left.\begin{array}{l} \text{T}\dfrac{dx}{ds} = \text{H} \\ \text{T}\dfrac{dy}{ds} = \text{V} - \Sigma_0^{x_1}\ \text{P} \\ \text{T}h\dfrac{dx}{ds} = \text{V}^{x_1} - \Sigma_0^{x}\ \text{P}x \end{array}\right\} \quad . \quad . \quad (16)$$

From (16) we may determine the mechanical conditions of the cable such as tension and deflection.

For instance suppose the load is uniformly distributed over the span, and

Let $\text{L} =$ the length of span,

$w =$ the load on a unit of length, and take the origin at the lowest point.

Then $\text{V} = \frac{1}{2} w\,\text{L}$; $\Sigma_0^{x^1}\ \text{P} = w\,(\frac{1}{2}\,\text{L} - x)$. These substituted in the second of (16) and then differentiated gives

$$d\,\text{T}\frac{dy}{ds} = w\,dx \quad . \quad . \quad . \quad . \quad (17)$$

The value of T from the first of (16) substituted in (17) gives

$$d\,\text{H}\frac{dy}{dx} = w\,dx \text{ the integral of which}$$

is
$$x^2 = \frac{2\text{H}}{w}y \quad . \quad . \quad . \quad . \quad (18)$$

which is the equation of a parabola.

Equation (18) aids us in using the third of equation (16), but the latter is rarely necessary, when the equation of the curve is known.

Equation (18) and the third of (16) give the same value for H at the lowest point. For at the lowest point $x = \frac{1}{2}\,\text{L} = x_1$, $y = h = \text{D}$

$$\frac{dx}{ds} = 1 \qquad \Sigma_0^{x}\ \text{P}x = \tfrac{1}{2}w\text{L} \times \tfrac{1}{4}\text{L} = \tfrac{1}{8}w\text{L}^2$$

$$\text{V} = \tfrac{1}{2}\,w\text{L}$$

which in the third of (16) gives

$$T = H = \frac{\frac{1}{4} w L^2 - y w L^2}{D} = \frac{w L^2}{8D} \quad . \quad . \quad (19)$$

which is the same that we obtain directly from (18). The same result may be obtained geometrically.

We would proceed in a similar way if the loading follows any other law. If the loading be the cable itself—forming a catenary—we have $V - \Sigma_0^{x_1} P = \int \partial K ds$, when ∂ is the weight of a unit of volume and K the section of the cable.

If the load increases uniformly from the middle each way, then $V_0^{x_1} \Sigma - P = \frac{1}{2} w x^2$

13°. If the lower chord be horizontal and the upper one convex upwards, we have $i_1 = 0, F_1 = H_1$ in (10), which will give

$$\left.\begin{aligned} F \cos i + F_2 \sin \theta &= H_1 \\ F \sin i + F_2 \cos \theta &= V - \Sigma_0^{x_1} P \\ H_1 h &= V x_1 - \Sigma_0^{x_1} P x \end{aligned}\right\} \quad . \quad . \quad . \quad (20)$$

The equations thus far deduced are for the most general distribution of the load. Let us now suppose that the load is uniformly distributed over a part or the whole of the span, being continuous from one end to the point considered,—or from end to end,—as the case may be. If the load extends from end to end we readily have

$$\left.\begin{aligned} V &= \tfrac{1}{2} w L; & \Sigma_0^{x_1} P &= w x_1 \\ V x_1 &= \tfrac{1}{2} w L x_1 & \Sigma_0^{x_1} P x &= \tfrac{1}{2} w x_1^2 \end{aligned}\right\} \quad . \quad . \quad . \quad (21)$$

If it extends from the remote end to the point considered—thus giving the maximum shearing,—we have

$$\left.\begin{aligned} V &= \frac{w(L - x_1)^2}{2L} & \Sigma_0^{x_1} P &= 0 \\ V x_1 &= \frac{w(L - x_1) x_1^2}{2L} & \Sigma_0^{x_1} P x &= 0 \end{aligned}\right\} \quad . \quad . \quad . \quad (22)$$

It is impossible to strictly realize the latter condition, for the weight of the frame always forms an appreciable part of the load, and may be considered a permanent load, while the surcharge may be called *moveable* or *transient.*

These may be considered separately or together. If separately, we shall have for the case of maximum shearing arising from the surcharge, from (20)

$$\left.\begin{aligned} F \cos i + F_2 \sin \theta &= H_1 \\ F \sin i + F_2 \cos \theta &= V \\ H h &= V x_1 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (23)$$

The 1st and 2d of (23) are the same as (3) and (2) page 224 of the April number of the *Journal* for this year.

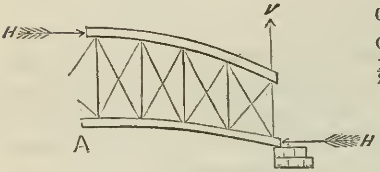
The weight of the frame cannot be definitely known until the strains upon the several parts are known, but these cannot be known until the load is known; hence the strains due to the weight of the frame remain an implicit function of the weight. We may, however, approximate to the weight by assuming that it is an uniform load, and then finding the sections of the principal parts at the middle of the frame. This assumption will not be far from the truth, and will generally be on the side of safety. I give the following as one method of finding the approxmate weight.

Let K = the section of the upper chord at the middle which also equals the section of the lower one,
L = the total length of span,
D = the depth of frame at the centre.
δ = the weight of a unit of volume of the frame,
n = the ratio between the weight of a chord and the whole weight of the frame.

Then the weight of the frame equals $n\delta KL$

and $V = \frac{1}{2} n\delta KL$

Fig 10



Let a section be made at the middle of the frame, and taking A as the origin of moments and we have

$\frac{1}{2} n\delta KL \times \frac{1}{4} L$ = the moment of the frame,

$\frac{1}{8} wL^2$ = the moment of the surcharge.

Hence the equation

$$HD = \frac{1}{8} n\delta KL^2 + \frac{1}{8} wL^2$$

$$\therefore \quad H = \frac{n\delta KL^2 + wL^2}{8D} \qquad \ldots\ldots \quad (24)$$

But this horizontal force must be resisted by the material. If C be the resistance to crushing of a unit of section, we have

$$CK = H = \frac{n\delta KL^2 + wL^2}{8D}$$

$$\therefore \quad K = \frac{wL^2}{8CD - n\delta L^2} \qquad \ldots\ldots \quad (25)$$

For cast iron $\delta = 0{\cdot}25$ lbs. per cubic inch,
for wrought iron $\delta = 0{\cdot}27$ lbs. per cubic inch,
for wood $\delta = 0{\cdot}03$ lbs. per cubic inch,

and n may be 3, 4, 5, or 6, according to the nature of the structure.

Although the weight of the frame may be considered an uniform load, yet it produces very different strains in different structures. For instance, if the upper chord be a parabola, so much of the chord as is really an uniform load produces no strains upon the ties and braces; while the lower chord and ties strain the ties and braces.

If the chords are parallel they produce the same or *very nearly* the same, strains as an uniform surcharge. It is *safe* to consider the effect in most practical cases the same as for an uniform surcharge, and I shall so consider it hereafter.

Although the load may be continuous, yet it is really supported at the joints where the ties and braces are connected with the chords; and the same effect would be produced upon the trussing if the load were divided into as many equal weights as there are bays, and each weight were supported directly by the ties and braces. This hypothesis will enable us to slightly modify the preceding equations. They may also be modified for many forms of trusses so as to be more convenient for computation. These modifications will form the subject of the next article.

(To be Continued.)

MECHANICS, PHYSICS, AND CHEMISTRY.

Note on the Variations of Density produced by Heat in Mineral Substances. By Dr. T. L. Phipson, F.C.S.

From the London Chemical News, No. 235.

That any mineral substance, whether crystallized or not, should diminish in density by the action of heat, might be looked upon as a natural consequence of dilation being produced in every case and becoming permanent. Such diminution of density occurs with idocrase, Labradorite, feldspar, quartz, amphibole, pyroxene, peridote, Samarskite, porcelain, and glass. But Gadolinite, zircons, and yellow obsidians augment in density from the same cause. This again may be explained by assuming that, under the influence of a powerful heat, these substances undergo some permanent molecular change. But in this note I have to show that this molecular change is not permanent, but intermittent, at least as regards the species I have examined, and probably with all the others. Such researches, while tending to elucidate certain points of chemical geology, may likewise add something to our present knowledge of the modes of action of heat. My experiments were undertaken to prove an interesting fact announced formerly by Magnus—namely, that specimens of idocrase after fusion had diminished considerably in density without undergoing any change of composition: before fusion their specific gravity ranged from 3·349 to 3·45, and after fusion only 2·93 to 2·945. Having lately received specimens of this and other minerals brought from Vesuvius in January last by my friend Henry Rutter, Esq., I determined upon repeating this experiment of Magnus. I found, first, that what he stated for idocrase and for a specimen of reddish-brown garnet was also the case with the whole family of garnets as well as for the minerals of the idocrase groupe; secondly, that it is not necessary to melt the minerals: it is sufficient that they should be heated to redness without fusion, in order to occasion this change of density; thirdly, that the diminished density thus produced by the action of a red heat is not a permanent state, but that the specimens, in the course of a month or less, resume their original specific gravities. These curious results were first obtained by me with a species of lime garnet in small yellowish crystals, exceedingly brilliant and resinous, almost granular, fusing with difficulty to black enamel, accompanied with very little leucite and traces of grossular, and crystallized in the second system. Specimens weighing some grammes had their specific gravity taken with great care, and by the method described by me in the *Chemical News* for 1862. They were then perfectly dried and exposed for about a quarter of an hour to a bright red heat. When the whole substance of the specimen was observed to have attained this temperature, without a trace of fusion, it was allowed to cool, and when it had arrived at the temperature of the atmosphere, its specific gravity was again taken by the same method as before. The diminution of density being noted, the specimens were carefully dried, enveloped in several folds of filtering paper, and put aside in a box along with other minerals. In the course of a month it occurred to me that it would be interesting to take the specific gravity again, in order to ascertain whether it had not returned to its original figure, when, to my surprise, I found that each specimen had effectively increased in density and had attained its former specific gravity. Thus:

Lime garnet mellitite (from Vesuvius).

	Original Density.	Density after being heated red-hot for a quarter of an hour and allowed to cool.	Density determined in a month after the experiments.
I.	3·345	2·978	3·344.
II.	3·350	2·980	3·350.
III.	3·349	2·977	3·345.

The same experiments were made with several other minerals belonging to the idocrase and garnet family, and always with similar results. Now I ask, what becomes of the heat that seems to be thus shut up in a mineral substance for the space of a month? The substance of the mineral is dilated, the distance between its molecules is enlarged, but these molecules slowly approach each other again, and in the course of some weeks resume their original positions. What induces the change, or how does it happen that the original specific gravity is not acquired immediately the substance has cooled?* Will the same phenomenon show itself with other families of minerals or with the metallic elements? Such are the points which I propose to examine in the next place; in the mean time the observations I have just alluded to are proof that bodies can absorb a certain amount of heat not indicated by the thermometer (which becomes latent), and that this is effected without the body undergoing a change of state; secondly, that they slowly part with this heat again until they have acquired their original densities; thirdly, so many different substances being affected by a change of density when melted or simply heated to redness and allowed to cool, it is probable this property will be found to belong, more or less, to all substances without exception.

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* Some minerals, like euclase, that become electric by heat, retain that state for a considerable time. The increase of density of Gadolinite and the decrease of density of Samarskite by action of heat, are accompanied by a vivid emission of light, as mentioned in my work on "Phosphorescence," &c., pp. 31 and 32, where H. Rose's ingenious experiment is described.

Proceedings of the Royal Society, May 26, 1864.

On the Pressure of Steam at high Temperatures. By R. A. Peacock, C.E.

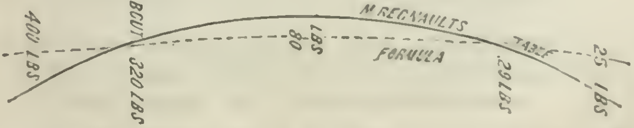
From the London Artizan, Feb., 1864.

(Continued from page 124.)

The reader has now the means of forming his own opinion as to the degree of reliance which can be placed on the formula. That is to say, if he accepts Dr. Fairbairn's temperature with the respective pressures of 56·7 and 60·6 lbs, and discards M. Regnault's temperatures, which are about ⅜ of a degree Fahr. greater for equal pressures, then the formula will be as accurate as could be expected or even wished for. Because we should have as follows:—

Pressure.		Temperature. Fahrenheit.	Formula.	Differences.
lbs. per sq. in.				
30	M. Regnault	250·23	250·17	+ ·06
56·7	Dr. Fairbairn	288·25	288·20	+ ·05
60·6	Ditto	292·53	292·48	+ ·05
300	M. Regnault	417·50	417·32	+ ·18
336·3	Dr. M. Rankine*	428·00	428·05	— ·05

These differences are exceedingly small. But if on the other hand, any reader rejects Dr. Fairbairn's experiments aforesaid and adopts M. Regnault's table throughout, the formula will be valueless to him for pressures greater than about 400 lbs to the square inch. Because M. Regnault's table gives a temperature less than the formula by ·24 for 25 lbs, this differance gradually diminishes with increasing pressures until at 29 lbs the table and the formula are exactly equal. With higher pressures the table gives temperatures in excess of the formula, which excess gradually increases until it attains its maximum of ·73° Fahr. at 80 lbs, after which the excess gradually diminishes, until the table and formula would be identical at about 320 lbs pressure. After which the formula would be in excess of the table, which excess would be greater and greater, until, as aforesaid, at about 400 lbs. pressure, the formula would cease to be of any value to the reader in question. These variances will be more readily apprehended by considering the following diagram, where the full line represents the temperatures in M. Regnault's table, and the dotted the temperatures of the formula:—



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* This is supposing Dr. M. Rankine's to be an experiment and not a calculation.

By which it will be seen that the formula, after about 400 ℔s, will necessarily diverge more and more from the table. The diagram shows the *manner* of the discrepancy, greatly exaggerated in *degree.*

The formula is as follows:—It has been stated that the temperature increases as the $4\frac{1}{2}$ root of the pressure in ℔s per square inch. Now for a pressure of 29 lbs. to the square inch both M. Regnault's table and formula give the same temperature, viz: 248·3°, let it therefore be assumed accordingly as a basis to commence from. We shall then have by the formula, supposing the temperature to give a pressure of 300 lbs. is required—

$$\frac{\text{log. of 29 lbs.}}{4{\cdot}5} : \text{log. of } 248{\cdot}3^\circ :: \frac{\text{log. of 300 lbs.}}{4{\cdot}5} : \text{log. of } 417{\cdot}32^\circ$$

which is the temperature required, as per table appended hereto. But this process may be shortened. The following is the working formula and gives the same results with half the labor, which is of importance where the calculations are numerous. By it the following table was calculated. No basis is required, the temperature is ascertained at once by adding the constant log. 2·07, which is equivalent to adding the second term and deducting the first.

Working formulas.—When the temperature to produce a given pressure is required:—

Rule.—Divide the log. of the given pressure by 4·5, and add log. 2·07; the sum is the log. of the temperature required.

Example—Required, the temperature to produce a pressure of 29 ℔s per square inch?

$$\frac{\text{log. of 29 ℔s.}}{4{\cdot}5} + 2{\cdot}07 = \text{log. of } 248{\cdot}3^\circ \text{ the temperature required.}$$

Or when the pressure produced by a given temperature is required, we have only to reverse the operation. Thus, let it be required what pressure per square inch will a temperature of 417·32° Fahr. produce? Here we have

$$\text{log. of } 417{\cdot}32^\circ - \text{log. } 2{\cdot}07 \times 4{\cdot}5 = \text{log. of } 299{\cdot}93 \text{ lbs.}^*$$

the pressure required.

Or, again, let it be required what will be the pressure of steam of a temperature of 3000° Fahr. which Sir W. G. Armstrong assumed as the temperature of subterranean fusion in his address to the British Association.

$$\text{log. of } 3000^\circ = 3{\cdot}4771213 - 2{\cdot}07 = 1{\cdot}4071213 \times 4\tfrac{1}{2} = 6{\cdot}3320458 =$$
$$2{,}148{,}050 \text{ lbs.} = 959 \text{ tons per square inch,}$$

if the formula can be depended on so far, and it would be difficult to prove either an affirmative or a negative, if we adopt Dr. Fairbairn's experiments.

It thus appears that by first ascertaining the temperature of saturated steam in a boiler, we at once get the pressure. Professor Daniell, F.R.S., ascertained the heat of a common fire to be 1141°

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* The correct temperature by the rule for 300 ℔s. is not 417·32°, but 417·322 +°.

Fahr.; this would give by the formula a pressure of upwards of 12 tons per square inch. Now, supposing an ordinary boiler, made of $\frac{3}{8}$ boiler plate; of course the strength of the plates would be reduced in strength at the joints by the rivet holes, and additionally by the length of the boiler, if that was considerable—short boilers being much stronger than long ones—a plate here and there would also be sure to be defective. All this being considered, there is no reason for surprise that such boilers should burst where the pressure may be of any amount up to, and even exceeding, 12 tons per square inch. At the same time, it is probably quite true that explosions sometimes take place from other causes than excessive pressure of steam directly on boiler plates.

Dr. Fairbairn's experiments on the tensile strength of rivet iron prove that iron may be heated up to about 400° Fahr. without impairing its strength.* This temperature gives a pressure of 250 lbs. per square inch. Now, if steam can be conveniently and economically heated up to this point (which ought not to be exceeded), it follows that boilers might be made much smaller than they are at present, where the pressure often does not exceed one-tenth part of 250 lbs. In that case, of course, the boilers must be made much stronger; they might, in fact, be made of thin armor plates of Bessemer's steel, joined together with double rows of rivets first, and then be properly bound together with steel bars.

To ascertain the steam pressure correctly, the following plan might be adopted. Provide a coiled steel spring suitably strong, carrying an index, and contained in a case or cavity like an ordinary letter weigher. Place actual weights of different amounts on the top of the brass piston, and graduate accordingly. This will prevent mistakes about the actuality of the pressure indicated. Insert the top of this piston of one inch square, on which the steam is to act, through the top plate of the boiler, and fix the instrument firmly there in an inverted position, the index and spring being outside the boiler. The piston may work through a stuffing box. In like manner a thermometer may be affixed, the bulb being inside the boiler. Thus the pressure and temperature can both be read off. The pressure, as has been said, must not be allowed to get above the 250 lbs., so that the boiler would be quite safe from bursting. This plan would apparently settle the question of boiler explosions; and, the boiler being small, it would not be out-of-the-way expensive, notwithstanding its being made of steel and unprecedentedly strong. In this case the specific volume of the steam, or the ratio of the volume of the steam to that of the water which produced it, would be, according to Dr. Fairbairn's formula, 123·76.

For the purpose of exhibiting in the following table that the temperature gradually increases with the pressure, which ought clearly to be the case if the calculations are correct, the whole of the calculations are given. This will also enable any one to make a comparison between the best known experiments and the calculations.

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* "Useful Information for Engineers," second series, p. 124.

TABLE of Pressures and corresponding Temperatures of Saturated Steam, calculated on the theory that the temperature increases as the 4½ *root* of the pressure, and conversely that the pressure increases as the 4½ *power* of the temperature.

Pressure.	Calculation.	Pressure.	Calculation.	Pressure.	Calculation.
lbs. per sq. in.	Temp. Fahr. deg.	lbs. per sq. in.	Temp. Fahr. deg.	lbs. per sq. in.	Temp. Fahr. deg.
25	240·24	53	283·90	145·8	355·50
26	242·34	54	285·08	147	356·14
26·5	243·37	55	286·25	150	357·75
27	244·39	55·9	287·28	154·3	360 (1)
27·4	245·19	56	287·40	160	362·91
27·6	245·59	56·7	288·20	163·3	364·56
28	246·37	57	288·53	165	365·40
28·83	247·98	58	289·65	170	367·84
29	248·30	58·8	290·53	170·99	368·31
29·4	249·05	59	290·75	180	372·54
30	250·17	60	291·84	182·4	373·63
31	252·01	60·4	292·27	190	377·04
31·458	252·83	60·6	292·48	195	379·22
32	253·81	65	297·08	200	381·37
33	255·53	66·15	298·23	203·3	382·75
33 1	255·71	67·23	299·31	210	385·52
33·71	256·74	69·21	301·25	220	389·52
34	257·23	70	302·01	225	391·48
35	258 90	73·5	305·30	225·9	391·82
36	260·52	75	306·67	230	393·39
36·75	261·70	79·03	310·26	240	397·13
37	262·11	80	311·10	250	400·75
37·8	263·36	80·85	311·83	252·619	401·68
38	263·67	85	315·32	254·089	402·20
39	265·57	88·2	317·92	255	402·52
39·25	265.57	89 86	319·24	260	404·26
40	266·70	90	319 35	270	407 67
40·3	267·14	95	323·22	272 008	408·33
41	268·16	95·516	323·61	280	410·97
41·7	269·17	95·55	323·63	285	412 60
42	269·60	100	326·92	290	414·19
42·196	269·88	101·9	328·28	300	417·32
43	271·02	102·9	329·01	315	421·87
44	272 40	105	330·49	316·858	422·42
44·1	272·54	108·4198	332·84	330	426 25
45	273·77	110	333·92	336·3	428·05
45·49	274·43	110·25	334 08	345	430·48
45·7	274·70	115	337·24	351·8298	432·36
46	275·11	115·1	337·30	360	434·58
47	276·42	120	340·44	375	438·54
48	277·72	124·95	343·51	400	444·87
49	279·	125	343·54	420	449·72
49·4	279·50	129·8	346·43	440	454·40
50	280·25	130	346·55	450	456 67
51	281·49	132·3	347·90	460	458·91
51·45	282 02	135	349·47	480	463·27
51·7	282·34	139·65	352·11	500	467·49
52	282·70	140	352·30	520	471·58(2)
52·52	283·33	145	355·06	540	475·55

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[1] Soft solder, two parts tin and one part lead, melts at 360°.

[2] Bismuth melts, 471·6°. (Dixon on heat.)

Pressure.	Calculation.	Pressure.		Calculation.	Pressure.		Calculation.
lbs. pr sq.in.	Temp. Fahr. deg.	lbs. pr sq.in.		Temp. Fahr. deg.	pr sq.in.		Temp. Fahr. deg.
					Tons.	lbs.	
550	477·49	1900		628·94	8½		1049·63
560, ¼ ton	479·41	1984		635 (2)	8¾		1056·41
580	483·16	2000		636·15	9		1063·05
600	486 82	2055		640 (3)	9¼		1069·54
620	490·38	2100		643·09	9½		1075·90
635	492·99	2200		649·77	9¾		1082·13
650	495·56	2240, a ton		652·37	10		1088·23
660	497·24	Tons.	lbs.		10¼		1094·22
680	500·55	1	152	662 (4)	10½		1100·10
700	503·78	1¼		685·54	10¾		1105·86
720	506·95	1½		713·89	11		1111·53
740	510·04	1¾		738·77	11¼		1117·09
750	511·57	2		761·02	11½		1122·56
760	513·08	2¼		781·20	11¾		1127·94
780	516·04	2½		799·71	12		1133·23
800	518·96	2½	73	802 (5)	12¼		1138·43
820	521·81	2½	332	810 (6)	12	839	1141 (8)
840	524·61	2¾		816·82	12½		1143·50
850	526	3		832·77	12¾		1148·60
860	527·36	3¼		847·72	13		1153·56
880	530·06	3½		861·80	13¼		1158·46
900	532·71	3¾		875·11	13½		1163·28
920	535·33	4		887·75	13¾		1168·03
940	537·89	4¼		899·79	14		1172·72
950	539·16	4½		925·37	14¼		1177·34
960	540·41	4¾		932·30	14½		1181·90
980	542·90	5		932·88	14¾		1186·40
1000	545·34	5¼		943·05	15		1190·84
1050	551·28	5½		952·85	15¼		1195·22
1100	557·01	5¾		962·31	15½		1199·54
1120, ½ ton	559·25	6		971·45	15¾		1203·82
1200	567·89	6¼		980·30 (7)	16		1208·04
1279·09	576· (1)	6½		988·88	17		1224·42
1300	578·08	6¾		997·21	18		1240·07
1400	587·68	7		1005·31	19		1255·06
1500	596·76	7¼		1013·17	20		1269·45
1600	605·38	7½		1020·84	25		1334
1680, ¾ ton	611·98	7¾		1028·30	30		1389·15
1700	613·59	8		1035·58	50		1556·14
1800	621·43	8¼		1042·69	100		1815·28

[1] Lead melts, 576°.

[2] Iron, red heat in the dark, 635°.

[3] Linseed oil boils, 640°.

[4] Mercury boils, 662°.

[5] Charcoal burns, 802°.

[6] Antimony melts, 810°.

[7] Iron, dull red heat, 980°.

[8] Heat of a common fire, 1141°

The following are the several melting heats of some of the more refractory metals, with the pressures of steam of equal temperatures calculated by the formula:—

Calculations of Pressure in Tons per square inch.		
114 . .	Brass melts at . .	1869°[11] Fahr.
115 . .	Silver " . .	1873°[12] "
153 . .	Copper " . .	1996°[13] "
237 . .	Gold " . .	2200°[14] "
326 . .	. . . . .	2360°[15] "
687 . .	Cast iron " . .	2786°[16] "
959 . .	Subterranean fusion .	3000°[17] "

The temperatures marked [5], [7], [8], [9], [10], [11], [12], [13], [15], are on the authority of Professor Daniell, F.R.S. [15], the temperature 2360°, is stated by Dr. Macquorn Rankine, F.R.S., to be that about at which the water in an engine boiler would be totally evaporated. (*Artizan* Nov., 1863, p. 252.) [17] In Sir W. G. Armstrong's address to the British Association, at Newcastle (p. 9), he assumes the temperature of the subterranean fusion to be 3000° Fahr. The other melting points, &c., have been obtained from a small volume on steam, published by the late Mr. Weale.

Earthquakes, Volcanic Explosions and Upheavals of Strata.—There is another point of view in which this formula may possibly be not without interest to a class of scientific men, other than engineers. More than a century ago the Rev. John Michell, M.A., "conjectured" that steam might be the cause of earthquakes, and he reasons very ably at considerable length on the subject.* His idea, however, seems to have been dropped, except by a very few, by whom it is entertained only as one out of several conjectural causes. Now if, as must now and then happen, fissures open in the bed of the sea, by the action of earthquakes and close again after a few seconds or minutes, it follows that a large body of water will rush down and be imprisoned, and come in contact with the fused matter below. This water will necessarily be converted into steam, which will only remain quiescent as long as it is everywhere surrounded by a resistance greater than its own expansive force. If the formula approximates towards the truth, unless the resistance amounts to a thousand tons per square inch or thereabouts on every side, in certain cases an explosion will take place of sufficient force to account for an earthquake or volcanic eruption, as the case may be. In reading accounts of volcanoes and earthquakes, it will frequently be observed that *hot water* and *steam* are ejected, to say nothing of the *hot water* and *steam* which notoriously issue from boiling springs and geysers. The writer has made a considerable collection of such cases. There is then plenty of direct proof of the existence of steam in the bowels of the earth, and steam will not be idle if it can find any point of less resistance than its own expansive force. Therefore, in considering the cause or causes of earthquakes, volcanic explosions, and upheavals of strata, you cannot get rid of steam. For let us look from whatever point of view we will the pressure of saturated steam must be enormous long before it reaches the temperature of 3000 degrees.

SIR :—The figures quoted by Mr. Peacock, in *The Artizan* for January, [*Jour. Fr. Inst.*, vol. xlviii. page 123,] from a table of mine in *The Artizan* for November last, are not to be regarded as independent data respecting the temperatures corresponding to various pressures of high pressure steam. They are merely a few of the results of a formula which I deduced from M. Regnault's experiments, and published in 1849; and they may be regarded as practically identical with the results of those experiments. W. J. MACQUORN RANKINE.

Glasgow, January 1864.

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* Phil. Trans. R.S. 1760. Vol. II, p. 447, &c.

On the Supposed Nature of Air prior to the Discovery of Oxygen.
By George F. Rodwell, F.C.S.

From the London Chemical News, No. 217.

(Continued from page 259.)

5. *Boyle's Experiments continued.—Experiment* 17.*—A tube three feet long and a quarter of an inch in diameter, closed at one end, was filled with mercury, and inverted in a vessel of the same metal; the latter was placed within the air-pump receiver, and the tube passed air-tight through its cover; on exhausting, the mercury fell nearly to a level with that in the vessel; but the same level within and without the tube could never be attained, because when the exhaustion was carried on for some length of time, air leaked into the receiver in spite of every precaution. When a quart receiver was used, the first downward stroke of the piston withdrew sufficient air to cause the column to fall eighteen and a-half inches. The falling of the mercury was not, however, entirely due to the removal of pressure, because a minute quantity of air was always present in the space above the mercury, and by its expansion the mercury column would be depressed. The mercury was never boiled in the tube in these early experiments, hence there was always a small amount of air, which was mechanically retained by the mercury, and rose into the so-called vacuum, when the tube was inverted. Boyle states that, when the tube was inclined, the mercury never reached quite to the top of it, and when a hot iron was held near that part of the tube above the mercury, a slight depression of the column was apparent.

Experiment 18—The height of the mercury column in Torricelli's tube was found to vary from day to day, and it did not vary in conformity with the weather-glass. According to Boyle the variation may be caused by ebbings and flowings in the atmosphere, and by sudden changes in its height and density, produced by causes with which we are unacquainted. That changes do take place, he considers is proved by the fact, that the refracting power of the air varies; and miners had related to him that a certain steam, called by them a "damp,"† sometimes arises within mines, and possesses such "thickness" that it extinguishes candles; it may also happen, he says, that fumes ascend from the earth with such rapidity, that the air through which they pass is dragged upwards by them, and a consequent diminution of pressure on that part of the earth beneath the ascending currents, takes place.

Mr. Wren, being asked by Boyle to mention any experiment relative to the pressure of air which he would wish to be tried, said it would be of importance to observe if the height of the mercury column varied according to the tides, inasmuch as, if it did not do so, Des Cartes' theory, that tides are produced by the increased pressure communicated to the air by the moon at certain times of the day than at others, would be disproved. Boyle did not find the height of the column affected by the state of the tides.

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* This experiment, as mentioned in the third of these papers, was tried by a different but far more complete method by Pascal; we must bear in mind, however, that this treatise "On the Weight of the Mass of Air," was not published till 1663, whereas Boyle's "Physico-Mechanical Experiments," appeared in 1661. Pascal undoubtedly, made the experiment first, but Boyle was probably unaware of the fact.

† From the German "dampf,"—vapor, steam, fume.

Experiment 19.—Having observed, in experiment 17, to what extent a column of mercury could be made to fall on the removal of pressure, Boyle next tried the same experiment with a column of water, contained in a tube four feet long. On exhausting, the water fell to within a foot of the surface of the water in the vessel into which the open end of the tube dipped. When air was re-admitted into the receiver, water was driven violently to the top of the tube.

In the 21st experiment Boyle declares he will not say for certain "whether or no air be a primogenial body," incapable of conversion into water; for, although many have affirmed that when water is heated it becomes air, he has observed in chemical distillations that the vapor of water quickly returns to water; and, in the Musæum Kircherianum, Schottus states there is a hermetically sealed glass vessel half full of water, which had been sealed for fifty years, without the water becoming air. On the other hand, however, it appears that air can be produced, because, when a number of iron nails were placed in a mixture of oil of vitriol and water, Boyle found that a quantity of air was evolved, and, although this does not prove that water may become air, it proves, he considers, that air may be generated.

If, by any chance, Boyle had brought a candle near his newly-generated air, what a wide field would have been opened to him when he found that there were other airs differing from "the air;" but the possibility of there being different kinds of air does not seem to have entered his head; for even carbonic acid gas, although so totally different from ordinary air, was only distinguished by him as "thickened air."

Experiment 27.—A watch was suspended by a thread in the air-pump receiver; on exhausting, the ticking ceased to be heard; but when air was re-admitted, it was heard as distinctly as before exhaustion.

A bell was supported in the receiver by a bent stick, the ends of which pressed against the sides of the receiver; when the latter was exhausted, and the bell rung, the sound was distinctly audible; hence Boyle concluded that sound can be conveyed by some rarer medium than air.

This is one of many examples of the great disadvantages under which the early experimenters labored, and we must make every allowance for the false conclusions at which they sometimes arrived. When Boyle made this experiment, very little was known about the nature of sound; there were no previous experiments to tell him that the thread would not convey the vibrations to the receiver, whereas the stick would do so, and he, therefore, could have no reason for believing that there would be any difference in the result obtained, whether he suspended his sounding body by a thread or by a piece of wood, and yet perfectly contrary effects were produced, and he was thus led to a wrong conclusion.

Experiment 33—was made with a view of determining the pressure of air on a known area. The air-pump receiver was removed from the pump, and the piston forced to the top of the cylinder, the upper orifice of the latter (into which the shank of the stop-cock of the receiver fitted) being left open: weights were attached to the piston until it was drawn down. The piston was then forced to the top of the cylinder—the upper orifice of the latter closed—and weights were again attached until the piston was drawn down; the weights obviously represent the force necessary to overcome the pressure of the air, acting on the area of the piston + the force necessary to overcome the friction between the piston and pump barrel; the weight necessary to overcome the latter source of resistance having been previously determined, it was subtracted from the total weight, and it was thus found that 112 lbs. were competent to overcome the pressure of the air on the piston. The experiment was modified by drawing the piston to the bottom of the cylinder, against the pressure of the air, and seeing how much weight it would draw up—105 lbs. + the weight of the piston rod was raised.*

Experiment 34.—Reasoning by analogy from the fact that two bodies of equal weight in air, but of different bulk, when weighed in water no longer balance each other, Boyle conceived that two bodies of equal weight in air would not be so in a vacuum. He, therefore, balanced on the opposite ends of a balance, capable of turning with $\frac{1}{32}$d of a grain, a piece of cork and a piece of lead; the balance was placed in the air-pump receiver: on exhausting the cork was found to preponderate, but on re-admitting air, it continued to preponderate from some cause which Boyle was unable to discover.

Experiment 36.—It is obvious, Boyle writes, that if we could place a balance above the atmosphere, or in a vacuum, we might weigh a quantity of air in the scales of the balance, just as we weigh substances in the air. In order to put this idea in practice, he procured a small "glass bubble," about the size of a hen's egg, and sealed it hermetically. It was then fastened to one end of the beam of the balance used in experiment 34, and was counterpoised by a piece of lead. The balance was placed in the receiver; on exhausting, the vessel of air greatly preponderated, and by placing weights on the other scale of the balance, so as to drag down the air vessel, it was found that, in a vacuum, the air in the bubble weighed $\frac{3}{4}$ths of a grain. On one occasion, when the exhaustion had been continued for some time, the included air burst the bubble with violence.

Boyle here describes several experiments which he made in order to determine the relative weights of air and water. The determination had been previously made by several different methods. Galileo found water to be 400 times heavier than air; Ricciolus (by weighing a bladder of air in air),* 10,000 times; and Merseunus, 1356 times. Boyle's method was to heat a copper elopile of known weight to redness, stop up the orifice with wax, weigh, perforate the wax to allow air to enter, and weigh again; the elopile was then filled with water, and weighed. By this method water was found to be 938 times heavier than air.

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* The diameter of the piston was three inches; hence its area = 7·0686 square inches. The pressure of the air upon it would, therefore, be, in round numbers, 7·0686 × 15 lbs. = 106·020 lbs.

Boyle next determined the relative weights of water and mercury, and found the latter to be $13\frac{92}{121}$ times heavier than the former. The relative weights of air and mercury were deduced from the above data, and were found to be as 14,000 to 1. When in possession of these facts, Boyle was able to calculate that the height of the whole atmosphere, if we suppose it to possess throughout the same density as at the surface of the earth, would be seven miles; but, he writes, knowing as we do how readily air expands when pressure is removed from it, it is not improbable that the atmosphere may extend to a height of some hundreds of miles.

Experiment 37.—Boyle frequently observed that when exhaustion was commenced the inside of the air-pump receiver became opaque, and that on the re-admission of air the opacity disappeared. We now know this phenomenon to be produced by the deposit of minute particles of water, previously existing in the state of vapor in the air. The air in expanding performs work, and a certain amount of molecular motion is converted into mass motion; consequently, the aqueous vapor in the air is condensed. When air is re-admitted, the particles strike against the sides of the receiver, and the amount of mass motion previously produced is re-converted into the molecular motion which produced it; consequently, the deposited water again becomes vapor.

Experiment 40.—In order to see whether the rarity of the air in an exhausted receiver would prevent insects from flying, Boyle placed a bee and a fly in the receiver, on exhausting, they both fell down "as in a swoon."

Experiment 41.—A lark was placed in the receiver; on exhausting, it was seized with convulsions, and although air was quickly re-admitted, it failed to revive. The pump had been worked ten minutes.

I have mentioned in the previous paper that the descriptions which Boyle gives of his experiments are exceedingly prolix; we have a good example before us in this experiment, which we should, in the present day, describe as above, but which Boyle, after telling us how he procured the bird, and how, when placed in the receiver, it did "divers times spring up in it to a good height," describes as follows: "The vessel being hastily, but carefully clos'd, the Pump was diligently ply'd, and the Bird for awhile appear'd lively enough; but upon a greater exsuction of the Air, she began manifestly to droop, and appear sick, and very soon after was taken with as violent and irregular convulsions as are wont to be observ'd in Poultry when their heads are wrung off. For the Bird threw herself over and over two or three times, and dyed with her Breast upward, her Head downwards, and her Neck awry. And though upon the appearing of these convulsions we turn'd the stopcock, and let in the air upon her, yet it came too late; whereupon, casting our eyes upon one of those accurate Dyals that go with a Pendulum, and were of late ingeniously invented by the noble and Learned Hugenius, we found that the whole Tragedy had been concluded within ten minutes of an hour, part of which time had been imploy'd in cementing the cover to the Receiver."

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* It is obvious, as mentioned in the first of these papers (Chemical News, vol. viii. p. 115), that a bladder of air, weighed in air, weighs no more than the empty bladder.

We must certainly give Boyle credit for being an accurate observer and an experimenter who did not pass over the most minute occurrences without mention; but conceive what a memoir would be in the present day if it were spun out like the above account of one experiment! Why, the Royal Society would have to publish folios, and the *Philosophical Magazine* would become a thick quarto, and it would require half a lifetime to master one branch of science.

After the success of the lark experiment, Boyle placed a sparrow and a mouse in the receiver. On exhausting, they both died. In order to see whether death was produced by the "steams" from the lungs stifling the animals or from want of air, a mouse was enclosed in a large receiver. At the end of several hours it was alive and well, but died when the receiver was exhausted, proving that want of air was the cause which had produced the previous deaths.

Boyle here enters into a discussion as to the nature of Respiration. Many of the philosophers of his time believed that the sole use of respiration was the cooling and tempering the heat of the heart and blood; and this theory was not only admitted by several of the ancient writers, but by the Cartesians and numberless learned men. But, Boyle answers, fishes and frogs, and "divers cold creatures," have need of respiration; and in the above-mentioned experiments, in which animals were killed in rarefied air, it did not appear that the interior of the receiver was hotter than the external air.

Some believed that air passes from the lungs to the left ventricle of the heart, both to reduce its heat and "to generate spirits." Mæbius and Gassendus believed that the real use of the air in respiration is to ventilate the blood in its passage through the lungs, in which passage impure vapors are given off by the blood, and are removed by the air.

According to Boyle, the use of air in respiration may be explained by two methods; for, first, just as a flame is stifled when burnt for some time in a close vessel by the "fuligenous steams" which it generates, so the vital fire of the heart may require fresh air to prevent it from being extinguished; or the air in the lungs may admit into its pores the impure vapors of the blood and remove them from the lungs. "We know" he continues, "that air too much thickened is unfit for respiration, because in the lead mines of Devonshire 'damps' sometimes arise which extinguish candles, and suffocate the miners. Also, cellars in which a large quantity of new wine is set to work, men have been suffocated on account of the great thickness of the air."

That the air is "thickened" by respiration Boyle proved by enclosing a bird in a small closed vessel. After three-quarters of an hour the air was found to be unfit to support life; and we can quite understand that the air must be thickened, he says, if we admit, with Sanctorius, that the quantity of matter which leaves our bodies by "insensible transpiration" exceeds in weight everything else which is given off from the body.

Air too much "thinned" is also unfit for respiration, as was proved by the death of the animals in an exhausted receiver; and Acosta states that he found some difficulty in breathing on the summit of a lofty mountain in Peru.

Although Boyle is inclined to believe that one of the uses of air in respiration is to purify the blood, by removing vapors which it throws off during its passage through the lungs, yet he conceives that it plays some other part; and Paracelsus* had a similar idea, for he affirms that just as the stomach assimilates a certain amount of meat we eat, and rejects the remainder, "*so the lungs consume part of the air, and proscribe the rest.*"

Cornelius Drebbel,† also believed that only a certain portion of air is consumed in the lungs, and it was reported that he had actually discovered that portion. It had been confidently stated to Boyle that Drebbel constructed a vessel for James I. capable of carrying twelve rowers and several passengers beneath the surface of water, and that, so soon as it was apparent that the air in the vessel had become impure from respiration, Drebbel removed the stopper from a bottle, filled with some liquid, which, by its evaporation, quickly rendered the air pure, and suitable for the purpose of respiration. This fact had been mentioned to Boyle by the friend of a man who had traveled in the vessel, by several relations of Drebbel's, and by a physician who married his daughter.

Experiment 43.—When water, wine, or oil of turpentine were heated to ebullition, and placed in the air-pump receiver, the boiling recommenced on exhausting, and continued for some time; but hot salad oil could not be made to boil.

With this experiment Boyle concludes his letter, first apologizing to Lord Dungarvan for not relating a larger number of experiments, many of which he had thought of and had wished to try, "were it not," he writes, "that my Avocations are grown so urgent for my remove from the place where the Engine was set up, that I am put to write your Lordship this Excuse, Weary, and in an Inne which I take in my way to my Dearest Brother Corke."

The account of the forty-three experiments detailed by Boyle in the treatise we have been consideirng, extends over 207 pages; but this may well be imagined from the extract given above from experiment 41.

In this first of Boyle's Pneumatic Treatises, there are an immense number of recorded facts; and when we remember the great difficulty of carrying out the experiments, we cannot too highly praise the industry and perseverance of their author. It is difficult for us, with our improved philosophical apparatus, to conceive the amount of labor attendant on the trial of experiments, 200 years ago, when the few instruments which existed were to a greater or less extent imperfect. In the present day, if we wanted to make the last experiment mentioned above, we should place our vessel of hot water on the air-pump plate, cover it with a receiver, work the pump for a few minutes, and the experiment would be finished. In order to try the same experiment Boyle had to suspend a vessel of hot water in the receiver of his air-pump, and to cement on the cover of the receiver, and, while an assistant worked the piston, to alternately open the stop-cock of the receiver, close it, open the lateral valve, close it, again open the stop-cock, and so on, till the exhaustion was as complete as it could be; the whole apparatus was meanwhile shaken with every stroke of the pump, and leakage of air at the rather numerous joints was perpetually occurring.

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* Born 1493. Died 1541.

† Born 1572. Died 1634.

(To be Continued.)

For the Journal of the Franklin Institute.

On the Elements of Physical Work, Vis-viva, Force, Velocity, Time, Power, and Work. By JOHN W. NYSTROM.

The difference between *force*, *power*, and *work*, appears to be not very generally understood, as we often find the power employed and the work accomplished, confounded with each other, and that even by men of science, experience, and high standing; such as in the case of a steam hammer being said to "strike a blow of so many tons," which is substantially parallel with saying that a bushel can be so many feet in length. Professor Tyndall speaks of heat as if it was only motion, and he also converts heat into work. If motion can be converted into heat, and heat into work, then work must be convertible into motion; which is substantially the same as to convert gallons into square feet, or linear inches.

Although the physical constitution of heat is not yet known, one thing appears to be certain, namely, that heat, like work, is a function of at least three simple elements, of which velocity (motion) is one.

The heat question will be referred to at the end of the article, in the development of our subject.

In geometry we have three fundamental elements, expressed by the terms, *length*, *breadth*, and *thickness*, employed to represent to our mind the nature of those several properties of space; we have in like manner in physics, three fundamental elements, expressed by the terms *force*, *velocity*, and *time*, employed to convey to us the laws of physical work.

Quantity, is that which can be increased or diminished by homogeneous parts.

Element, is that which cannot be dissolved into two or more different quantities.

Function, is the product of two or more different elements.

Force, *Velocity*, and *Time*, are elements.

Power, *Space*, and *Work*, are functions.

Force, is the first element of work, like length in geometry. It is recognised as pressure, and measured by weight. Physical forces will herein be denoted by the letter F, and the weight of a mass or body by M.

Velocity, is the second element of work, like breadth in geometry. It is the continuous change of position, recognised as motion, and denoted by the letter V.

Time, is the third element of work, like thickness in geometry. It implies a continuous action, recognised as duration, and denoted by T.

Power is a function of the two first elements, *force* F, and *velocity* V, like area in geometry is a function of length and breadth. Force in motion produces power, like a line in motion produces a surface. Power cannot exist without the two elements force and velocity, as a surface cannot exist without length and breadth. Power is denoted by $P = FV$, measured and expressed in foot-pounds, which means a force of so many pounds moving a distance of so many feet per unit of time. One unit of power is "a force of one pound moving with a velocity of one foot per second." This unit or foot-pound has also been called effect. James Watt assumed the power of a horse equivalent to 33,000 pounds raised one foot in one minute, which is equal to 550 pounds raised one foot in one second, or 550 effects. The power of a horse is considered equivalent to that of eleven men, or the power of one man equal to 50 effects.

Space, is a function of the second and third element, *velocity* V, and *time* T, like a cross-section of a solid, is a function of breadth and thickness. Space is here denoted by $S = VT$.

Work (*vis-viva*) is the product of the three elements, *force* F, *velocity* V, and *time* T; and may be likened to a solid in geometry which has three dimensions. Work, denoted by $W = FVT$, is produced by the duration of power for a certain time, as a solid is produced by a plane in motion. Work is the product of power and time, like a solid is the product of area by thickness. *Work* cannot be produced without the three elements *force*, *velocity*, and *time*, as a *solid*, cannot exist without the three elements *length*, *breadth*, and *thickness*.

Work, appears to be the most confused function in physics, and has been denominated by a great many conventional terms, as *vis-viva*, *living force*, *energy*, *power*, *momentum*, *effect*, &c., &c. None of those terms express what is meant by the formulas assigned to them namely, MV^2, which means the physical work, concentrated in a moving mass, or the work required to set a body in motion.

When the formula is given, we need not care what it is called, if we only know what it is; but unfortunately when a function is denominated by a wrong and improper term, we are apt to misconceive or misapply its meaning, which has actually been the case with the improper terms for *power* and *work*.

The latin word Vis (force) and Viva (living) means "living force," which in fact is *power*, and expressed by FV; but when we exercise this living force for a certain duration of time, it becomes a different function, namely MV^2, which is *living power* or physical work, and not merely living force. It may now be remarked that, " a living force cannot exist without some duration of time, because a living force is a force in motion, and motion requires time."

A steam engine of 100 horse power, running one day, will produce a certain amount of work; let the same engine run for only half a-day when it will produce only half the work of a whole day, but the power was still 100 horses. Let it run for only an hour, a minute, a second, or for an infinitely short period of time, the engine is still 100 horses but did no work. For that infinite small space of time, the living force was there and produced the full power $F\,V = 100$ horses but did no work $M\,V^2$. In illustration of this, let a solid cube of paper represent the work $M\,V^2$; then the base of the cube, or the bottom sheet, which we will consider to have no thickness, will represent the power $F\,V$. Take away the sheets one by one from the top of the cube—which is to reduce the time or thickness—until only the bottom sheet is left; then there is no more solidity of the cube, and no more work $M\,V^2$ left, but the power or living force $F\,V$, represented by the base of the cube is undisturbed.

There are two reasons why the true meaning of physical work has hitherto been so obscured, namely, *first*, that the primitive formula for work $F\,V\,T$ or $M\,V\,T$, has been concealed by the formulas for the law of gravity, by which the time T has been transformed into velocity V, and the work represented by $M\,V\,V$ or $M\,V^2$. Sometimes the space S has been inserted for $V\,T$, when the work has been called foot-pounds $F\,S$. We have now in scientific works two kinds of foot-pounds, without proper explanation of their difference. The foot-pounds of power, means so many pounds raised so many feet per unit of time; while the foot-pounds of work, means so many pounds raised through a space S, independent of time because the time is included in the space $S = V\,T$.

The *second* reason why it has been obscured, is that we have no established unit by which to measure physical work. We speak about *vis-viva*, the same as we would speak about length without reference to miles, feet, or inches, but merely conceive it to be, long, longer, and longest; short, shorter, and shortest.

It is therefore found necessary in the development of the subject, to establish a unit for work, and assign to it a name.

Let us assume the work accomplished by one horse power in a time of one hour, to be the unit for physical work; which will be the same as that of eleven men working one hour, or that of one man working one day of eleven hours. In order to clearly distinguish this unit from that of power, let it be called a "WORKMANDAY," which means a man's day's work. Let us further designate the quantity of physical work, or number of *workmandays* with the letter W. A workmanday expressed by force and space, without regard to time or velocity, will then be $550 \times 3600 = 1980000$ foot-pounds, or 2564·7668 units of heat. At least we will adopt this unit, name, and signature, until better shall be established by higher authority. We can now understandingly proceed to develope the physical laws of the elements of work.

All kinds of work can be estimated in workmandays, such as building a house, steamboat, or bridge, digging a canal, ploughing the ground, throwing a bomb or projectile, fighting a battle, steam-boiler explosion, &c., &c.

Momentum is the product of two or more elements.

Static momentum is a function of force and lever, or force multiplied by the length of the lever it acts upon.

Dynamic momentum is power, or the force multiplied by its velocity.

Momentum of power is physical work, or power multiplied by time.

Momentum of physical work is the cost in money, or the work multiplied by its price.

Momentum of work of art is the product of physical work and the skill of the artist.

Momentum of work of science is the product of physical work by primitive knowledge.

Notation of Letters.

S = space in feet, passed through by the force F in the time T.

F = force or pressure, expressed in pounds avoirdupois.

V = velocity or motion, expressed in feet per second.

T = time of operation in seconds.

P = power in foot-pounds, of one pound raised one foot per second.

H = horse power of 550 pounds raised one foot per second.

W = physical work, expressed in workmandays, of 1980000 foot-pounds, or 2564·7668 units of heat.

M = weight in pounds, of a moving mass, or the weight of a mass acted upon by mechanical force.

g = acceleratrix of gravity, being 32·166 feet per second, the velocity which a falling body attains at the end of its first second.

G = acceleratrix of the combined gravity and mechanical force.

v = angle with the horizon of the direction of the moving force and mass, as on an inclined plane. When the direction rises above the horizon, use the top sign, and when it deeps under use the lower sign.

L = number of laborers employed (not workmandays).

D = number of days of 11 working hours.

N = number of horses (not horse power).

n = number of blows of a steam hammer, or pile-driver.

f = friction and other incidental elements opposing the force F, or moving mass M, in pounds.

Co-efficients $550 \times 3600 = 1980000$.
“ $32·166 \times 550 \times 3600 = 63688680$.

I beg to remark here, that with a binary arithmetic and metrology, the co-efficients 550 and 3600 would disappear in the formulas, and 28 of the following formulas would also be dispensed with, say nothing of the labor and confusion it would save.

The first formula gives the theoretical value, while the last ones containing f includes friction and other incidental circumstances.

Formulas for Mechanical Work.

Space in feet passed through in the time T.

$$S=VT. \qquad S=\frac{P T}{F}. \qquad S=\frac{550 T H}{F}. \qquad S=\frac{550\times3600 W}{F}.$$

Force or pressure in pounds.

$$F=\frac{P}{V}. \qquad F=\frac{550 H T}{V S}. \qquad F=\frac{550\times3600 W}{V T}. \qquad F=\frac{550\times3600 W}{S}.$$

Velocity in feet per seconds.

$$V=\frac{S}{T}. \qquad V=\frac{P}{F}. \qquad V=\frac{550 H}{F}. \qquad V=\frac{550\times3600 W}{F T}.$$

Time of duration in seconds.

$$T=\frac{S}{V}. \qquad T=\frac{S F}{P}. \qquad T=\frac{S F}{550 H}. \qquad T=\frac{550\times3600 W}{F V}.$$

Power in foot-pounds.

$$P=F V. \qquad P=\frac{F S}{T}. \qquad P=550 H. \qquad P=\frac{550\times3600 W}{T}.$$

Horse power.

$$H=\frac{P}{550}. \qquad H=\frac{F V}{550}. \qquad H=\frac{F S}{550 T}. \qquad H=\frac{3600 W}{T}.$$

Work, in workmandays.

$$W=\frac{F V T}{550\times3600}. \qquad W=\frac{F S}{550\times3600}. \qquad W=\frac{P T}{550\times3600}. \qquad W=\frac{H T}{3600}.$$

Number of laborers (not workmandays).

$$L=\frac{W}{D}. \qquad L=11 N. \qquad L=\frac{F V}{50}. \qquad L=\frac{F S}{550\times3600 D}.$$

Number of Horses (not horse power).

$$N=\frac{L}{11}. \qquad N=\frac{W}{11 D}. \qquad N=\frac{F V}{550}. \qquad N=\frac{F S}{11\times550\times3600 D}$$

Number of days of 11 hours each.

$$D=\frac{W}{L}. \qquad D=\frac{W}{11 N}. \qquad D=\frac{50 W}{F V}. \qquad D=\frac{F S}{550\times3600 L}.$$

Work, in workmandays.

$$W=D L. \qquad W=H D N. \qquad W=\frac{F V D}{50}. \qquad W=\frac{F^2 V S}{50\times550\times3600 L}$$

Examples for Mechanical Work.

EXAMPLE 1. A steam engine of $H=145$ horses is to hoist a weight of $F=34368$ pounds. Required how high $S=?$ will the weight be lifted in $T=25$ seconds.

3d. formula for space.

$$S = \frac{550\ TH}{F} = \frac{550 \times 25 \times 145}{34368} = 58 \text{ feet the answer.}$$

Example 2. How much coal $F = ?$ can a steam engine of $H = 36$ horses hoist up from a mine of $S = 256$ feet deep, in ten hours or $T = 36000$ seconds.

2d. formula for force.

$$F = \frac{550\ HT}{S} = \frac{550 \times 36 \times 36000}{256} = 2784375 \text{ pounds,}$$

or 1243 tons.

Example 3. A force of $F = 378$ lbs. moving uniformly through a space of $S = 3675$ feet in $T = 490$ seconds. Required how much power P it will exert.

2d. formula for power.

$$P = \frac{FS}{T} = \frac{378 \times 3675}{490} = 2635 \text{ foot-pounds.}$$

That is, 2635 lbs. will be moved 1 foot per second.

Example 4. The pressure on a steam piston is to be $F = 31350$ pounds. Required how fast it must move to generate a power of $H = 456$ horses.

3d. formula for velocity.

$$V = \frac{550\ H}{F} = \frac{550 \times 456}{31350} = 8 \text{ feet per second.}$$

Example 5. What horse power $H = ?$ is required to accomplish the same amount of work in $T = 43860$ seconds, that $W = 132$ workmen could accomplish in one day?

4th. formula for horse power.

$$H = \frac{3600\ W}{T} = \frac{3600 \times 132}{43860} = 10{\cdot}82 \text{ horse power.}$$

How much work $W = ?$ is accomplished by a steam engine of $H = 164$ horses, working $T = 3496$ seconds?

5th. formula for work.

$$W = \frac{HT}{3600} = \frac{164 \times 3496}{3600} = 159{\cdot}36 \text{ workmandays.}$$

or it would require 159·36 workmen for a whole day to do the same amount of work.

Example 6. The pressure on a steam piston is $F = 2864$ pounds, moving with a velocity of $V = 6$ feet per second during 10 hours, or $T = 36000$ seconds. Required how much work will be accomplished?

1st. formula for work.

$$W = \frac{FVT}{550 \times 3600} = \frac{2864 \times 6 \times 36000}{550 \times 3600} = 312{\cdot}44 \text{ workmandays.}$$

EXAMPLE 7. How many workmandays $W=?$ are required to hoist up $F=843640$ lbs. of iron to a height of $S=45$ feet?

2d. formula for work.

$$W=\frac{FS}{550\times3600}=\frac{843640\times45}{550\times3600}=1917{\cdot}3 \text{ workmandays.}$$

EXAMPLE 8. What time is required for an engine of $H=96$ horse power, to raise a weight $F=495000$ lbs. to a height of $S=48$ feet?

3d. formula for time.

$$T=\frac{SF}{550H}=\frac{48\times495000}{550\times96}=450 \text{ seconds.}$$

Formulas for Work under the action of Gravity.

Space passed through in the time T.

$$S=\frac{gT^2}{2}=\frac{VT}{2}. \qquad S=\frac{PT}{2M} \qquad S=\frac{4\times550^2H^2}{2gM^2}. \qquad S=\frac{550\times3600W}{M}.$$

A mass in motion, or falling vertically.

$$M=\frac{2\times550\times3600W}{gT}. \quad M=\frac{550\times3300W}{S}. \quad M=\frac{2\times550H}{\sqrt{2gS}}. \quad M\frac{550\times3600Wg}{V^2}.$$

Velocity at the end of the time T in feet per seconds.

$$V=gT=\frac{2S}{T}. \quad V=\frac{2\times550H}{M}. \quad V=\sqrt{2gS.} \quad V=\sqrt{\frac{550\times3600gW}{M}}.$$

Time of duration in seconds.

$$T=\frac{2\times550H}{gM}. \quad T=\sqrt{\frac{2S}{g}}. \quad T=\sqrt{\frac{550\times3600\times2W}{gM}}.$$

Power in foot-pounds.

$$P=\frac{MTg}{2}. \quad P=\frac{MV}{2}. \quad P=\frac{M2S}{T}. \quad P=550\times3600W\sqrt{\frac{g}{2S}}.$$

Horse power.

$$H=\frac{MTg}{2\times550}. \quad H=\frac{M\sqrt{2gS}}{2\times250}. \quad H=\frac{MV}{2\times250}. \quad H=\frac{3600W}{T}.$$

Work, in workmandays.

$$W=\frac{MV^2}{g550\times3600}. \quad W=\frac{MS}{550\times3600}. \quad W=\frac{P\sqrt{\frac{2S}{g}}}{550\times3600}. \quad W=\frac{H\sqrt{\frac{2S}{g}}}{3600}.$$

Number of laborers (not workmandays), $L=\frac{MSn}{550\times3600D}$.

Working days of 11 hours each, $D=\frac{MSn}{550\times3600L}$.

Number of horses, (not horse power), $N=\frac{MSn}{11\times550\times3600D}$

Work in workmandays, . $W=\frac{MSn}{550\times3600}$.

Examples for Work under the action of Gravity.

EXAMPLE 9. A steam-hammer weighing $M = 13450$ lbs falls from a height of $S = 7$ feet. Required how much work the hammer can do in each blow?

2d. formula for work.

$$W = \frac{MS}{550 \times 3600} = \frac{13450 \times 7}{550 \times 3600} = 0{\cdot}04755 \text{ workmandays,}$$

and $0{\cdot}04755 \times 11 = 0{\cdot}523$ hour.

That is, one man working 0·523 of an hour can accomplish the same work as that of each blow of the steam-hammer. The horse power of the hammer in its fall is found by the second formula, but this is not the horse power acting on the mass of iron to be forged on the anvil. If we know the time from when the hammer first touched the iron until it stops, then the horse power could be found by the fourth formula. The cooler the iron is, and the more resistance presented to the blow, the greater will the horse power be; but the correct measure of the capacity of a hammer is the workmandays, or its weight multiplied by the height of its fall.

EXAMPLE 10. A steam-hammer of $M = 10000$ lbs falling a height of $S = 4$ feet. Required the average horse power acting in the fall?

2d. formula for horse power.

$$H = \frac{W \sqrt{2gS}}{2 \times 550} = \frac{10000 \times \sqrt{2 \times 32{\cdot}16 \times 4}}{2 \times 550} = 164 \text{ horses.}$$

EXAMPLE 11. From what height $S = ?$ must a mass $M = 31800$ lbs fall to accumulate a work of $W = 3{\cdot}6$ workmandays.

4th. formula for space.

$$S = \frac{550 \times 3600\,W}{M} = \frac{550 \times 3600 \times 3{\cdot}6}{31800} = 224{\cdot}15 \text{ feet.}$$

EXAMPLE 12. How much work is accumulated in a mass of $M = 26800$ lbs and with a velocity of $V = 60$ feet per second?

1st. formula for work.

$$W = \frac{MV^2}{g\ 550 \times 3600} = \frac{26800 \times 60^2}{32{\cdot}166 \times 550 \times 3600} = 1{\cdot}515 \text{ workmandays.}$$

or it would require one man to work $1{\cdot}515 \times 11 = 16{\cdot}66$ hours, to wind up the mass M so high that it would in its fall attain the velocity V.

EXAMPLE 13. Required how high $S = ?$ the mass M in the preceding example, must be raised, in order to attain a velocity of $V = 60$ feet per second in its fall?

3d. formula for space.

$$S = \frac{V^2}{2g} = \frac{60^2}{2 \times 32{\cdot}166} = 55{\cdot}96 \text{ feet the answer.}$$

EXAMPLE 14. What mass $M = ?$ can be raised up $S = 68$ feet by $W = 16$ workmandays.

2d. formula for mass.

$$\text{M}=\frac{550\times3600\text{W}}{\text{S}}=\frac{550\times3600\times16}{68}=465882 \text{ pounds.}$$

Example 15. How much work is required to raise $\text{M}=3137800$ pounds, to a height of $\text{S}=80$ feet.

2d. formula for work.

$$\text{W}=\frac{\text{MS}}{550\times3600}=\frac{3137800\times80}{550\times3600}=12{\cdot}678 \text{ workmandays.}$$

Example 16. How many laborers are required to do the work $\text{W}=12{\cdot}678$, in $\text{D}=3$ days.

$$\text{L}=\frac{12{\cdot}678}{3}=4{\cdot}226 \text{ laborers.}$$

Example 17. How many laborers $\text{L}=?$ are required to strike $n=1000$ blows in $\text{D}=\frac{1}{2}$ or half-a-day with a pile-driver? the ram weighing $\text{M}=2000$ lbs. and lifted $\text{S}=1{\cdot}5$ feet in each blow.

$$\text{L}=\frac{\text{M S }n}{550\times3600\times0{\cdot}5}=\frac{2000\times1{\cdot}5\times1000}{550\times3600\times0{\cdot}5}=3 \text{ men.}$$

Example 18. How many days are required for $\text{N}=2$ horses to draw up from a mine $\text{M}=18948000$ lbs. of coal? the depth of the mine is $\text{S}=250$ feet.

$$\text{D}=\frac{\text{M S}}{550\times3600\times11\text{N}}=\frac{18948000\times250}{550\times3600\times112}=47{\cdot}295 \text{ days.}$$

To be continued on physical work by the combined action of the force of gravity and mechanical force; also on physical work generated by the action of heat.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

From the London Mechanics' Magazine, April, 1864.

[Report of Chief Engineer, *April 26, 1864.]

During the last month 199 engines have been examined, and 313 boilers, 8 of the latter being examined specially, and 1 of them tested with hydraulic pressure. Of the boiler examinations, 253 have been external, 9 internal, and 51 thorough. In the boilers examined, 140 defects have been discovered, 7 of them being dangerous. Of some of these defects, a few further particulars may be given, not that there is anything very novel about them, but because it is just those practical defects that are of common occurrence which it is important—and the more so on account of their very frequency—to note and avoid. It is thought, therefore, that a statement of the defects constantly met with in the boilers inspected will be of service, not to the steam user only, but also to the boiler setter and maker. Four dangerous cases of fracture all occurred at the seams of rivets at the bottom of

*Error page 246—for "May 2," read "April 5."

externally fired boilers, as well as six others, which were serious though not actually dangerous. In three of these cases the injury manifested itself in leakage, and in cracks at the rivet holes, some of them extending for as much as 12 inches into the plate. In the fourth instance, one of the plates over the fire rent completely through from one end to the other, at a transverse line of rivet holes, the rent gaping open about $\frac{3}{4}$ of an inch. In this instance the boiler was nearly new, having only been laid down about six months, and had not been severely pressed during that time, while the character of the workmanship was good. The feed water, however, was introduced at the furnace end of the boiler, and too near to the plates over the fire, while it was of a sedimentary character; and though it did not form a tenacious scale, deposited a good deal of powder, which was so fine as to be almost impalpable; in addition to which, the boiler was not fitted with any blow-out apparatus. It is to these causes that the failure is attributed, although it should be added that the feed was heated before its introduction to the boiler by the exhaust steam from the engine. From the fact of the boiler being new, the workmanship good, the service was not very severe, and the feed water heated, it might have been supposed that, if any externally-fired boiler could escape fracture, this one would have done so, and its failure affords another illustration of the necessity, so frequently pointed out in previous reports, of introducing the feed to these externally-fired boilers, by means of a horizontal perforated pipe at a distance from the furnace, and also of adopting an efficient blow-out apparatus for the prevention of deposit. There is little question that these precautions would have prevented the injury in the present instance. A boiler lately put under the charge of the Association was found on the first "Thorough" examination to be so eaten away at the bottom over the mid-feather wall, on which it was set that a considerable portion of the plates were reduced to half of their original thickness; fortunately the diameter of the boiler was small, so that there was a considerable margin of strength. It is, however, another of those constantly recurring instances of the danger of setting boilers on mid-feathers.

One instance of dangerous grooving occurred to a multitubular boiler, and at the transverse seams at the bottom, the plates being so seriously reduced in thickness that the inspector made two or three holes through them at the time of his examination. It should be stated that this boiler had external brick flues, which had been supplemented since it was originally laid down. Instances of external grooving similar to the above are of very frequent occurrence in multitubular boilers, owing to the straining of the seams of rivets at the bottom, which is induced by the various temperatures of different parts in consequence of the imperfect circulation of the water; while it may be added that three other cases have been met with during the past month which, though not at present actually dangerous were still serious, and the corrosion steadily progressing.

Two Butterley boilers, put under the charge of the Association during the last month, were found to have been worked with steam at as high a pressure as 40 lbs, although they were 8 feet in diameter, made of plates three-eighths of an inch in thickness, and 15 years old.

One case of deficiency of water occurred during the night time to an internally-fired boiler, which was one of a series connected together, and under the charge of the watchman, who though firing-up to raise steam so as to be ready for starting by six o'clock, appears to have been perfectly oblivious to the fact of their being no water in the gauge glass. The two fusible plugs, however, with which the boiler was fitted, both acted, and thus saved the furnace crowns from injury. The water had been lost in the following way:—All the steam junction valves were screwed down, and the pressure of steam in the boiler in question happened to be higher than that in the adjoining one, and thus the water had been driven from one into the other. This would not have been possible, even in spite of the inequality of steam pressure, had the feed back pressure valves been rightly constructed. But the valve and the spindle were not detached, but linked together by a collar at the end of the latter, so that although the valve was self-acting within a certain range, one turn of the screw beyond that point held it off from the face, and rendered it no longer a self-acting feed back-pressure valve; and hence the loss of water just referred to. The danger of such a mode of construction will at once be seen, and the importance of having the valve and spindle *entirely distinct* one from the other, and *not linked together in any way.* It is recommended that all those members whose valves are of similar construction to the one which permitted the loss of water referred to above, should have them remedied at once.

An explosion took place on the 21 of February last, of which the particulars could not be obtained at the time. The engineer has been supplied with a copy of the report drawn up by the two engineers appointed to examine the boiler and to investigate the cause of the explosion, the report being accompanied with a drawing showing the construction of the boiler, as well as the position and character of the rents. From these sources of information the following particulars have been taken:—It appears that the boiler was of Cornish construction, and internally-fired, having one furnace tube, which was of oval shape. The shell was 26 ft. in length, and 6 ft. in diameter, while the oval flue was 3 ft. 10 ins. width, and 3 ft. 4 ins. in height. The oval shape of this flue necessarily made it very weak, more especially so, since the major axis was horizontal, and the crown consequently flat. Were internal furnace-tubes subjected to statical pressure only, it would be immaterial whether the major axis in those of oval shape were horizontal or vertical, but in consequence of the columnar action which is induced by the expansion of the plates by the action of the fire, and which especially affects the upper part of the tubes, the conditions become altogether altered, and those tubes that have flattened crowns are particularly liable to collapse. On account of its oval shape this tube had been stayed with cast iron flanged pipes placed within it, which acted as pillars or struts in resisting the external pressure.

These pipes were spaced at irregular distances, set vertically and fixed from the inside of the tube. The boiler is reported to have been fitted with a Bourdon steam-pressure gauge, also suitable water gauges and an adequate safety-valve, loaded to 22 lbs on the square inch. The flue collapsed at about the middle of its length, and rent at the crown through one of the transverse seams of rivets, the plates being forced down till they touched the bottom of the flue; while, in addition, the upper half of the furnace mouth angle iron had rent from the front plate. Also two of the cast iron pillars had been dislodged, one of them being found after the explosion lying within the flue near the rupture, and the other a few yards in front of the boiler, a piece being burnt off the end of it, while the pillar was evidently too short for the purpose for which it had been intended. The preceding statement of facts will leave no room to doubt, that the cause of the explosion was simply the insufficiency of the boiler, especially when it is added that old cracks about 5 feet long, were found running along the sides of the flue, and which had been patched with plates barely one-quarter of an inch in thickness. The boiler was evidently malconstructed in the first place, and its condition neglected in the second, and hence the explosion, which competent inspection could not have failed to prevent. For the past month one explosion is reported, by which one man was killed and three others injured. It occurred at a considerable distance from Manchester, and consequently has not been personally investigated. The boiler in question, which was of marine construction, was not under the inspection of the Association.

[Report of the Chief Engineer, May 31, 1864.]

During the last month 226 engines have been examined, and 402 boilers, 16 of the latter being examined specially, and 1 of them tested with hydraulic pressure. Of the boiler examinations, 287 have been external, 18 internal, and 97 thorough. The following defects have been found in the boilers examined:—Furnaces out of shape, 2 (1 dangerous); fracture, 8 (2 dangerous); blistered plates, 2; internal corrosion, 11; external corrosion, 9 (3 dangerous); internal grooving, 8; external grooving, 4; feed apparatus out of order, 1; water gauges ditto, 9 (1 dangerous); blow-out apparatus ditto, 9; fusible plugs ditto, 2; safety-valves ditto, 1; pressure gauges ditto, 11; deficiency of water, 1; total, 78 (7 dangerous). Boilers without glass water gauges, 5; without pressure gauges, 2; without blow-out apparatus, 22; without back pressure valves, 22.

Six explosions occurred during the past month, by which 11 persons have been killed, and 13 others injured. None of the boilers were under the inspection of this Association, while all of them, with one exception, have been personally examined since the explosion. One of these, by which one person was killed and two others injured, occurred to the boiler of a locomotive engine, while standing at a short distance from a railway station. The locomotive, which was employed on goods traffic, and had six wheels coupled, was of the ordinary construction, and about 10 years old, having been re-tubed two years since. The cylindrical portion of the boiler, as well as the crown of the outer fire-box shell, was rent into a number of pieces; and but little of the portion of the boiler shell between the fire-box and the smoke-box left; while many of the tubes were fractured, and the remainder bowed outwards. The fragments were scattered in every direction, and some of them blown to a great distance, one of the spring balances belonging to the safety-valves falling through the roof of a cottage, and seriously injuring one of the inmates. The cause of the explosion was attributed at the inquest by three successive witnesses, all of whom were practical boiler-makers, to shortness of water and consequent overheating of the plates. This, it was thought had produced a gas inside the boiler, which, upon ignition from the hot tubes caused the explosion. For this, it was added, the driver, who had been killed by the explosion, was responsible, in having allowed shortness of water to occur. The jury brought in a verdict of accidental death, adding, that they could not clearly ascertain the reason of the explosion. A personal examination of the exploded boiler led to a very different conclusion from that given in the preceding evidence. The crown of the inner fire-box, as well as its sides, was found uninjured, and the roofing stays in perfect condition; while the rents, instead of being in the fire-box, which would have been the case had the plates been overheated, were confined to the external shell, as already described; so that it is clear that the explosion was not due to shortness of water. On further examination, it was found that the character of the water was somewhat corrosive, and the surfaces of some of the plates had been eaten into indentations by it; while close to the overlap at a longitudinal seam of rivets, at the left hand side of the cylindrical portion of the boiler, and in the ring of plates nearest to the smoke-box, a deep furrow was found to have been eaten, which ran longitudinally more than half-way across the width of the plate. At this furrow, which was about the water-line, the plate had rent, and there can be but little question that this rent was the primary one from which the other developed, and to which the explosion had been due; so that the supposition of shortness of water, as well as that of the formation of explosive gases, and the neglect of the engine driver, may be dismissed, and the explosion attributed simply to weakening of the plates through the furrowing action, which is found to be more or less developed in nearly all boilers, but more especially in locomotives.

The next, like the preceding one, occurred to the boiler of a locomotive engine, while standing very near to the station. The locomotive was employed on passenger traffic, was of the ordinary construction, and about ten years old. The shell rent at the base of the steam dome, which was shot upwards, and thrown to a considerable distance. From this opening a number of rents radiated, and the upper part of the cylindrical portion of the shell was rent into several pieces, which, however, did not fly to any great distance, and but little damage resulted from the explosion altogether. The consideration of the directions in which the fragments had flown, as well as an examination of the parts themselves, left little room to question that the primary rent had occurred at the base of the steam dome, where the ring of angle iron, with which it was attached to the shell, was found to be severed for a considerable distance through the line of rivet-holes. This explosion is considered, therefore, to have been due to the existence of the steam dome, and that, had it not been for this, the explosion would not have occurred. The danger of these steam domes has been frequently pointed out to the members of the Association; and this explosion, which is by no means singular, affords an additional illustration of the importance of dispensing with them altogether, at all events, in stationary boilers. A good deal of misconception, however, exists with regard to their efficiency; and they are considered by some to make a valuable addition to the amount of steam room within the boiler, and thus to compensate for irregular loads upon the engine. The fallacy of this, however, may be simply shown. Few steam domes exceed by more than four times the capacity of the engine cylinder, so that, even if the dome could be entirely exhausted, it would work the engine but a few strokes; while the entire space above the water line in a Lancashire boiler, 7 ft. in diameter and 30 ft. long contains but little more steam than the amount generated every minute when in ordinary work. Again, one foot of chamber room, filled with steam at 60 ℔s, will only yield 4 ft. at atmospheric pressure; whereas, 1 cubic foot of the water, which would be of a temperature of 309°, Fahr., would yield considerably above 100 cubic feet of steam at atmospheric pressure. So that in a boiler worked at a pressure of 60 ℔s a foot of space, filled with water, has stored up within it more than thirty times as much power as one filled with steam. From this it will be clear that the reservoir of power is not in the steam, but in the water. In order to prevent priming, all that is necessary is to take the steam off at a number of points, and not to allow a concentrated rush at any one. To effect this, horizontal perforated pipes, carried along within the steam space, which are so widely known, are found to be most efficacious. A considerable number are at work in the boilers under inspection and give very satisfactory results.

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[Report of the Chief Engineer, June 28, 1864.]

During the last month 272 engines have been examined, and 419 boilers, 22 of the latter being examined specially, and one of them tested with hydraulic pressure. Of the boiler examinations, 364 have been external, 8 internal, and 47 thorough. In the boilers examined, 154 defects have been discovered, 2 of them being dangerous.

The particulars of the explosions, which were postponed from the last monthly report, may now be given. None of the boilers in question were under the inspection of this Association; while they have all been personally examined since their explosion, and the cause of the occurrence investigated. No. 12 explosion resulting in the death of one person, and in serious injury to another, occurred at an iron-works to a plain cylindrical egg-ended boiler, fired externally, and not under the inspection of this Association.

The boiler was one of a series of five connected together and working side by side, being No. 4 from the left hand. Its length was 40 ft., its diameter 6 ft., and the thickness of the plates ⅜ of an inch.; while the pressure of the steam was 35 lbs, which was quite moderate for a boiler of such dimensions. It had rent into eleven fragments, which were scattered in every direction, one of them being thrown to a distance of 200 yards. The character of these rents was peculiar. The majority of boilers of this class divide into two parts at one of the transverse seams of rivets, but this one had not only rent transversely, but also longitudinally, from one end to the other, so as to divide the boiler in the main into four nearly equal parts; while these were again subdivided, and the shell ultimately broken up into the eleven pieces just named. These rents were by no means confined to the lines of rivets, but had run through the solid plates entirely regardless of them, in many cases continuing for several feet within a few inches of the overlaps, and though so near, yet without running into them, but continuing in a straight line parallel to them. Indeed, there was scarcely a line of rivets disturbed, and some of the smaller fragments were torn out of the heart of the larger plates without a single rivet upon them.

The boiler had been originally plated longitudinally, but on the seams over the fire giving way some time since, it had been repaired with three widths of plate laid transversely. These plates, which were 3 ft. wide each, extended to a short distance behind the fire bridge, and it was at the ring seam of rivets that connected the new plating laid transversely, with the old laid longitudinally, that the primary rent occurred, and which it will be seen was situated, as is so usual in these cases, near to the bridge, and at the bottom of the boiler; while the anomalous manner in which the boiler had rent was due to the combination of the transverse and longitudinal modes of plating. The manager of the works informed me that their externally-fired boilers were a source of constant annoyance and expense, through getting out of repair, and it was no uncommon thing for one of the ring seams, a little behind the fire bridge, suddenly to rend through the line of rivet holes, merely in consequence of the slight change of temperature induced on the stoker's cleaning out the fires with the door open. The fact of these externally-fired boilers being ever found to give way in this treacherous manner, seems to me a sufficient reason to condemn them, especially at iron-works where the value of the charge of metal in the blast furnaces, which far exceeds that of the boilers, is jeopardized by them. That the remaining boilers in the series, or the furnaces themselves, escaped in this instance, is a piece of peculiar good fortune, since, in other cases, the whole range of boilers has been instantaneously destroyed, and the blast furnaces themselves demolished.

No. 13 Explosion took place at a colliery. In this instance three persons were killed, and three others injured; while the boiler, which was not under the inspection of this Association, was the outer one of a series of three, and, as in the case of the previous explosion, was of plain cylindrical egg-ended construction, and externally-fired. The boiler, which was plated longitudinally throughout, was 32 ft. long, 6 ft. 6 ins. in diameter, and made of plates $\frac{3}{8}$ of an inch in thickness, the pressure of steam being 35 lbs per square inch. The primary rent occurred at a longitudinal seam of rivets over the fire, which after running in a straight line for some feet developed transversely, dividing the shell into three fragments, all of which were thrown to a considerable distance from their original seating; while in addition, the adjoining boiler was dislodged, and turned up on end by the force of the explosion. When it is stated that the plates at the fractured part proved to be very defective, and also that this boiler had leaked for some time at the seams over the fire, so that the introduction of bran had been resorted to in order to stop it, it will not be necessary, after what has been said on the danger of these externally-fired boilers, to add anything further to account for this explosion; while it will appear that these boilers, whether plated longitudinally, as in the present instance, or transversely, as according to the more usual practice, are alike prone to explosion.

No. 15 Explosion by which one man was killed, was due to the collapse of the combustion chamber of a boiler of the double furnace or breeches class, working at a flour-mill, and which was not under the inspection of this Association.

The boiler was a left hand one of a series of three, the shell being 7 ft. in diameter, and 26 ft. 6 ins. long; while the diameter of the furnaces was 2 ft. 9 ins., and that of the flue 3 ft. 3 ins.; the length of the combustion chamber being 4 ft. 6 ins.; the thickness of the plates $\frac{3}{8}$ of an inch, and the steam pressure 45 lbs. The boiler had been built by a first-class maker, and the quality of the material as well as the character of the workmanship appeared to be good throughout.

The rush of steam and water from the rent in the combustion chamber drove the boiler about 6 ft. forward into the firing space, and although the other boilers remained unmoved, yet the steam pipe connexions to them were broken. In consequence of this, the steam and hot water escaping from the junction valve on the centre boiler, played with fatal effect into the firing space and engine room. The engineer and stoker, who were there at the time, made a rush at the first sound of the collapse, to a door which opened out on the river running directly alongside of the boiler-house, and the tide being out, jumped down upon the bed. The engine-man happily escaped with a few bruises, but the stoker, less fortunate, was so severely scalded that he died the same night. The collapse of the combustion chamber had taken place, not at the crown but at the underside, and this arose from the fact that while the crown was stiffened with roofing stays, assisted by tie rods connected to the shell of the boiler, the bottom of the combustion chamber was comparatively unstayed, having but a single angle iron running longitudinally on the centre line, in addition to a small gusset on each side. These breeches or combustion chambers have already proved a very fruitful source of explosion, and it is important that any steam users, who are employing boilers of this construction, should have these chambers stayed with vertical water tubes which act as internal columns or struts, and thus prevent the top and bottom plates of the chamber coming together; while, in addition, it is frequently if not always desirable, that the flue should be encircled with an angle iron hoop just at the waist or termination of the breeches piece. In some cases, where the pressure is low, this hoop of itself would be sufficient, and under many circumstances would perhaps be more easily obtained than the water tubes. Had these precautions been adopted in the boiler under consideration, the collapse of the breeches or combustion chamber, and the consequent explosion, would have been prevented.

Sir John Herschel on the French and English Standards of Measurement.

From the Lond. Civil Eng. and Arch. Jour., July, 1864.

We extract the following letter to the editor from a recent number of the *Times:*

In the *Times* of Thursday, the 16th inst., I observe a letter addressed by Mr. Ewart to the editor of the *Journal des Debats* on the subject of the introduction into this country of the French metrical system. In this letter Mr. Ewart appears to be at a loss to understand why the *Times* has all along raised its voice in opposition to the proposed change; and speaks of the prevalent repugnance to its adoption as arising merely from British prejudice, which he considers to be gradually yielding to the efforts making by himself and his coadjutors to enlighten us as to its advantage.

It is well for those who have adopted a contrary opinion, whether from the exercise of their own judgment, or from a perusal of the many able expositions of the confusion and inconvenience the change would create which have from time to time appeared in your columns, to be enabled to render an account of the faith that is in them; and you will excuse me if I observe that, in the articles referred to, I nowhere find any clear and distinct statement of those reasons which appear to myself decisive against the change on purely scientific grounds, and which lead me to regard it, scientifically considered, as a retrograde step. These, with your permission, I will endeavor to state as briefly as may be, for the benefit of that very large portion of the public who may not have read my recent remarks on the subject in one of our most widely circulated monthly periodicals.

Whatever be the historical origin of our standards of weight, capacity, and length, as a matter of fact our British system refers itself with quite as much arithmetical simplicity, through the medium of the inch, to the length of the earth's polar axis (a unit common to all nations) as the French does through that of the metre to the elliptic quadrant of a meridian passing through Paris (a unit peculiar to France). It does so as regards our actual legal standards of weight and capacity with much more precision than the French system, and as regards that of length (with a correction which, if legalized, would be absolutely imperceptible, from the smallness of its amount, in any transaction of life, and which can be applied, *currente calamo*, almost without calculation to any statement of lengths) with even still greater, and indeed with all but mathematical, exactness.

If the earth's polar axis be conceived divided into five hundred million inches, and a foot to be taken to consist of 12 such inches, then one hundred of our actual legal imperial half-pints by measure, or one thousand of our actual imperial ounces by weight, of distilled water at our actual standard temperature of 62° Fahr., will fill a hollow cube having one such foot as its side. The amount of error in either case is only one part in 8000.

The theoretical French metre is one ten-millionth part of the elliptic quadrant above mentioned; the theoretical litre is one-thousandth of a cubic metre; and the theoretical gramme, one millionth part of a cubic metre of distilled water at 32° Fahr. The actual error of the French legal or standard litre or gramme, or the deviation of these standards as they actually exist, from their true theoretical value, is one part in two thousand seven hundred and thirty (2730), and is consequently relatively nearly three times as great as the error in our standards of capacity and weight when referred to the earth's polar axis as their theoretical origin in the manner above stated.

Our actual imperial measures of length deviate, it is true, by more than this amount from their theoretical values so defined; that is to say, by one part in one thousand; so that a correction of one exact thousandth part subtracted from the stated amount of any length in imperial measures suffices to reduce it to its equivalent in such units as correspond to similar aliquots of the polar axis; a correction performed if needed, as already remarked, *instanter*, and *currente calamo* requiring no tables and almost no calculation. So corrected the outstanding error is only one part in sixty-four thousand (64,000). The actual legal metre in use in France is however, not immaculate in this respect, its amount of error being one part in six thousand four hundred (6400), which is ten times that which our British measures so corrected would exhibit.

If it were worth while to legalize so trifling an alteration (and were an Act passed rendering permissive the decimalization of our own system, it would be necessary to do so as a means of bringing the national units of length, weight, and capacity, into exact decimal correspondence), no mortal would be aware, practically speaking, that any change had been made in our mile, yard, foot, or inch. I have in common use two foot rules bought at respectable shops, and neither the worse for wear, which differ by more than the amount of change required.

The writer in the *Journal des Debats* is pleased to say that England stands in extensive commercial relations with only one considerable nation (North America) employing the British system of weights and measures. British commerce extends, however, to Russia, British India and Australia, all of them superior in area, and the two last, at least, of equal importance, commercially speaking, with the totality of the metricised nations. The Russian sagene is an exact multiple of the English foot (imperial). The hath (the legal measure of length in British India) is 18 imperial inches. The Australian system is identical with our own. Taking into consideration this immense preponderance, both in area, in population, and in commerce, we are not only justified in taking our stand against this innovation, but entitled to inquire, if uniformity be insisted on, why, with an equally good theoretical basis (to say the least), the majority is called upon to give way to the minority.

J. F. W. HERSCHEL.

Collingwood, June 18.

For the Journal of the Franklin Institute.

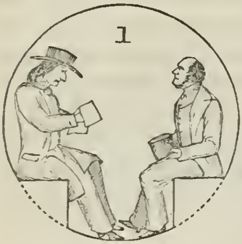
Atmospheric Pressure as a Traveling Force, versus *Animal Power and Steam Locomotives.*

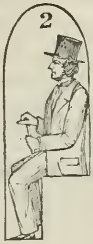
(Continued from page 279.)

It may readily be supposed that to establish Pneumatic passenger lines, the dimensions of the tunnels or tubes should be as small as possible, not only because their cost swells greatly with their bore, but because the larger one is the larger are the volumes of air to be drawn out, and the longer the time and greater the outlay of force to do that. This may be said to depend on the arrangement of the passengers seats. In common cars it matters little how they are placed, in the direction of the length or breadth of the cars, but in an air tube the difference is very great. Every addition to its width enlarges its capacity and cost. A company of soldiers can march in single file through a passage less than three feet wide, but not in squads of three or four abreast; so a pneumatic tunnel adapted for a single row of passengers can pass the same number through as a much larger one seated across it, and in the same time. If length be thus substituted for breadth, the result would be a tube, the cost of which would be reduced to a minimum, and, as it would seem, the expense of working it also.

What then is the form and size of such a tube? We assume that one whose section is a circle five feet in diameter could receive two rows of passengers seated *vis à vis* as in fig. 1, with the space under the seats excluded. The sitting posture would be uneasy for tall travelers and there would be a lack of head-room on going in and out. This would suggest the drawing in of the sides so as to form an ellipse by dividing of which vertically and taking half of it, we should have, with a slight modification, the second figure. But though here taken for a section of a tube of the smallest dimensions it can only be considered as that of the car, to which the tube proportionately enlarged would have to conform.

It is, however, obvious that so severe an economy of space and such a disposition of travelers would militate against, if they did not prove fatal to the system. The comforts and conveniences of travel would be materially lessened, social and confidential converse, except with adjoining individuals, would be out of the question, while almost every one would be cramped for want of room and change of position. Traveling in a single file cannot become a general feature of atmospheric lines, nor is it so essential in an economical point of view, as has just been supposed. The reasoning in the first paragraph is plausible and would be conclusive if a liquid instead of air had to be pumped out. It is to some extent superficial. Almost as much may be advanced in favor of the proposition that in the working of a line little is gained by a small tube over a large one.





Imagine two laid down of equal length, one having a sectional area twice that of the other, equal loads linked to the pistons, and each operated on by a like amount of exhausting power: would not the trains start together and reach the end of the routes at the same time, and with the same expenditure of force? While the larger one had a double quantity of air to remove, its density would only require to be half diminished. If the smaller piston had a pressure of 8 lbs. on the inch, that of the other would have 4 lbs. on an area twice as large. So that, throwing friction of the pistons out of view, as regards speed and power, they would be much alike, and accommodations in common cars are compatible with Pneumatic tunnels of moderate dimensions.

The principle by which the resistance is accommodated to the force and the force to the resistance, is one of the fluid phases of the lever. Its applications may be supposed in their infancy, and this pumping of men and merchandise, like water, through pipes, a foreshadowing of its future operations. E.

Protecting Armor Plating.

From the London Artizan, Sept. 1864.

Great success has attended the plan adopted for protecting the armor plating of the iron-cased wooden frigates, as applied in the first instance of the *Royal Oak*. This vessel has been admitted into the dock at Malta for the purpose of ascertaining the results of the measures adopted for preventing the effects of galvanic action on the armor plating. The plan, which was suggested by Mr. Reed, the chief constructor of the navy, consists in sheathing the plates with teak planking three inches in thickness, the inside of the planking and the surface of the armor plating being at the same time coated with Hay's water-proof glue, and the planking attached to the armor plates by short screws. Since the *Royal Oak* has been in dock at Malta a portion of the planking has been removed, and the results have been in the highest degree satisfactory, the armor plates having been perfectly protected, while not even the slightest trace of galvanic action can be detected. This improvement in the way of preserving the armor plating of wooden frigates is the more important when it is recollected how seriously the plates of the *Royal Oak* were found to have suffered only a very short time after the frigate had been launched, on the vessel being docked and inspected in this country. Mr. Reed's plan was subsequently applied to the *Ocean*, armor-cased frigate, at Devonport, and so satisfied are the Lords of the Admiralty with the results in the case of the *Royal Oak*, that they have ordered the whole surface of the armor below the water to be protected in the same way and the work will be carried out at Malta dock-yard.

For the Journal of the Franklin Institute.

Strength of Cast Iron and Timber Pillars: A series of Tables showing the Breaking Weight of Cast Iron, Dantzic Oak, and Red Deal Pillars. By Wm. Bryson, Civ. Eng.

(Continued from Vol. xlv., p. 186.)

Solid Uniform Square Pillars of Dantzic Oak, both Ends being Flat and Firmly Fixed.

Length or height of Pillar in feet.	Side of the square in inches.	Value of W in tons from formula $W = 10\cdot95 \frac{D^4}{L^2}$	Value of c.	Calculated breaking weight in tons from formula. $Y = \frac{Wc}{W + \frac{3}{4}c}$
8	6·2036	253·40	132·8	95·33
10	"	162·17	"	82·27
12	"	112·62	"	70·47
14	"	82·74	"	60·26
16	"	63·35	"	51·62
18	"	50·05	"	44·41
20	"	40·54	"	38·41
22	"	33·59	"	33·12
8	8·8622	1055·39	271·04	227·26
10	"	675·45	"	208·34
12	"	469·06	"	189·09
14	"	341·61	"	170·47
16	"	263·84	"	153·08
18	"	208·47	"	137·22
20	"	168·86	"	122·98
22	"	139·55	"	110·52
24	"	117·26	"	99·15
26	"	99·91	"	89·31
28	"	86·14	"	80·66
30	"	75·05	"	73·08

Solid Uniform Square Pillars of Red Deal, both Ends being Flat and Firmly Fixed.

		$W = 7\cdot81 \frac{D^4}{L^2}$		
8	6·5	217·83	124·21	87·00
10	"	139·41	"	74·45
12	"	96·84	"	63·30
14	"	71·12	"	53·77
16	"	54·45	"	45·82
18	"	43·02	"	39·24
20	"	34·85	"	33·81

Hollow Uniform Cylindrical Pillars of Cast Iron, both Ends being Rounded or Irregularly Fixed.

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Calculated breaking weight in tons from formula $W = 13\frac{D^{3.76} - d^{3.76}}{L^{1.7}}$	Value of w.	Value of c.	Calculated breaking weight in tons from formula. $Y = \frac{wc}{w + \frac{3}{4}c}$	Strength per sq. inch of section in tons.
1	2	1·5	6		116·40	67·34	46·96	34·16
1·25			7·5		79·65	"	41·21	
1·5			9		58·42	"	36·12	
1·75			10·5		44·95	"	31·71	
2			12		35·82	"	27·94	20·32
2·25			13·5		29·32	"	24·73	
2·5			15		24·51	"	22·00	
2·75			16·5		20·84	"	19·67	
3			18		17·98	"	17·68	12·86
3·25			19·5	15·69				
3·5			21	13·83				
3·75			22·5	12·30				
4			24	11·02				8·04
4·25			25·5	9·94				
4·5			27	9·02				
4·75			28·5	8·23				
5			30	7·54				5·48
5·25			31·5	6·94				
5·5			33	6·41				
5·75			34·5	5·95				
6			36	5·53				4·02
6·25			37·5	5·16				
6·5			39	4·83				
6.75			40·5	4·53				
7			42	4·25				3·10
7·25			43·5	4·01				
7·5			45	3·78				
7·75			46·5	3·58				
8			48	3·39				2·47
8·25			49·5	3·22				
8·5			51	3·06				
8·75			52·5	2·91				
9			54	2·77				2·01
9·25			55·5	2·65				
9·5			57	2·53				
9·75			58·5	2·42				
10			60	2·32				1·68
10·25			61·5	2·22				
10·5			63	2·13				
10·75			64·5	2·05				
11			66	1·97				1·43
11·25			67·5	1·90				
11·5			69	1·82				
11·75			70·5	1·76				
12			72	1·70				1·24

(Table continued.)

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Calculated breaking weight in tons from formula $W = 13\frac{D^{3.76} - d^{3.76}}{L^{1.7}}$	Value of w.	Value of c.	Calculated breaking weight in tons from formula. $Y = \frac{wc}{w + \frac{3}{4}c}$	Strength per sq. inch of section in tons.
1	2·5	2	4·8		230·85	86·59	67·57	38·24
1·25			6		157·97	"	61·45	
1·50			7·2		115·87	"	55·49	
1·75			8·4		89·16	"	50·09	
2			9·6		71.05	"	45·24	25·60
2·25			10·8		58·15	"	40·90	
2·5			12		48·62	"	37·07	
2·75			13·2		41·34	"	33·68	
3			14·4		35·66	"	30·69	17·36
3·25			15·6		31·12	"	28·05	
3·5			16·8		27·44	"	25·72	
3·75			18		24·40	"	23·65	
4			19·2		21·86	"	21·81	12·34
4·25			20·4	19·72				
4·5			21·6	17·90				
5·75			22·8	16·32				
5			24	14·96				8·46
5·25			25·2	13·77				
5·5			26·4	12·72				
5·75			27·6	11·80				
6			28·8	10·97				6·20
6·25			30	10·24				
6·5			31·2	9·58				
6·75			32·4	8·98				
7			33·6	8·44				4·77
7·25			34·8	7·95				
7·5			36	7·51				
7·75			37·2	7·10				
8			38·4	6·73				3·80
8·25			39·6	6·38				
8·5			40·8	6·07				
8·75			42	5·78				
9			43·2	5·50				3·12
9·25			44·4	5·25				
9·5			45·6	5·02				
9·75			46·8	4·80				
10			48	4·60				2·61
10·25			49·2	4·41				
10·5			50·4	4·23				
10·75			51·6	4·07				
11			52·8	3·91				2·22
11·25			54	3·77				
11·5			55·2	3·63				
11·75			56·4	3·50				
12			57·6	3·37				1·91

(Table continued.)

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Calculated breaking weight in tons from formula $W = 13\,\frac{D^{3\cdot76} - d^{3\cdot76}}{L^{1\cdot7}}$	Value of w.	Value of *c*.	Calculated breaking weight in tons from formula. $Y = \frac{wc}{w + \frac{3}{4}c}$	Strength per sq. inch of section in tons.
1	3	2·5	4		401·98	105·83	88·37	40·93
1·25			5		275·08	“	82·13	38·40
1·5			6		201·76	“	75·95	35·17
1·75			7		155·25	“	70·02	32·43
2			8		123·72	“	64·47	29·86
2·25			9		101·27	“	59·33	27·48
2·5			10		84·66	“	54·62	25·29
2·75			11		72·00	“	50·33	23·31
3			12		62·10	“	46·45	21·60
3·25			13		54·20	“	42·94	19·88
3·5			14		47·78	“	39·76	18·41
3·75			15		42·49	“	36·90	17·09
4			16		38·08	“	34·31	15·89
4·25			17		34·35	“	31·96	14·80
4·5			18		31·17	“	29·84	13·82
4·75			19		28·43	“	27·91	12·92
5			20	26·05				12·06
5·25			21	23·98				
5·5			22	22·16				
5·75			23	20·54				
6			24	19·11				8·85
6·25			25	17,83				
6·5			26	16·68				
6·75			27	15·64				
7			28	14·70				6·80
7·25			29	13·85				
7·5			30	13·07				
7·75			31	12·37				
8			32	11·72				5·42
8·25			33	11·12				
8·5			34	10·57				
8·75			35	10·06				
9			36	9·59				4·44
9·25			37	9·15				
9·5			38	8·75				
9·75			39	8·37				
10			40	8·02				3·71
10·25			41	7·69				
10·5			42	7 38				
10·75			43	7·09				
11			44	6·82				3·15
11·25			45	6·56				
11·5			46	6·32				
11·75			47	6·09				
12			48	5·88				2·72

(Table continued.)

Length or height of Pillar in feet.	External diameter in inches.	Internal diameter in inches.	Number of diameters contained in the length or height.	Calculated breaking weight in tons from formula $W = 13 \frac{D^{3 \cdot 76} - d^{3 \cdot 76}}{L^{1 \cdot 7}}$	Value of w.	Value of c.	Calculated breaking weight in tons from formula $Y = \frac{wc}{w + \frac{3}{4}c}$	Strength per sq. inch of section in tons.
1	3·5	3	3·428		635·34	125·07	108·98	42·73
1·25			4·285		431·78	"	102·87	40·54
1·5			5·142		318·90	"	96·64	37·89
1·75			6		245·38	"	90·18	35·48
2			6·857		195·55	"	84·52	33·14
2·25			7·714		160·06	"	78·85	30·92
2·5			8·571		133·81	"	73·52	28·83
2·75			9·428		113·80	"	68·55	26·88
3			10·285		98·15	"	63·95	25·07
3·25			11·142		85·66	"	59·69	23·40
3·5			12		75·54	"	55·79	21·87
3·75			12·857		67·16	"	52·18	20·46
4			13·714		60·18	"	48·88	19·16
4·25			14·571		54·29	"	45·85	17·98
4·5			15·428		49·26	"	43·06	16·88
4·75			16·285		44·93	"	40·50	15·88
5			17·142		41·18	"	38·15	14·96
5·25			18		37·90	"	35·99	14·11
5·5			18·857		35·02	"	34·00	13·33
5·75			19·714		32·47	"	32·16	12·61
6			20·571	30·21				11·84
6·25			21·428	28·18				
6·5			22·285	26·36				
6·75			23·142	24·72				
7			24	23·21				9·11
7·25			24·857	21·89				
7·5			25·714	20·67				
7·75			26·571	19·55				
8			27·428	18·52				7·26
8·25			28·285	17·58				
8·5			29·142	16·71				
8·75			30	15·90				
9			30·857	15·16				5·94
9·25			31·714	14·47				
9·5			32·571	13·83				
9·75			33·428	13·23				
10			34·285	12·67				4·96
10·25			35·142	12·15				
10·5			36	11·66				
10·75			36·857	11·21				
11			37·714	10·78				4·22
11·25			38·571	10·37				
11·5			39·428	9·99				
11·75			40·285	9·63				
12			41·142	9·29				3·64

(*To be continued.*)

For the Journal of the Franklin Institute.

Compressed Air and Gunpowder as Traveling Forces.

The first, though often proposed has never been adopted, and, as I think, for a sufficient reason. It is certainly desirable that rail cars on short routes should be worked if possible by a portable inorganic force, even at a somewhat greater cost than by draft animals. Could this be done with compressed air the achievement would be a great one. Besides leaving street pavements untouched and therefore incurring no expense for re-paving them, the capital invested in horses and stabling, feed and attendants, &c., would be transferred to a steam-compressing engine, at one or both ends of a line, and to motive machinery more lasting than living tissues and much cheaper to keep.

The future has many good things in store for us, and, possibly, this may be among them, for it does not appear to be within the compass of our present resources. Difficulties and dissatisfaction attending our city cars have suggested afresh the use of compressed air without a due consideration of what it is competent to do and what not. True, the reservoir or receiver of each car would be filled in a moment or two, either directly by the engine or from a common reservoir kept charged to the proper tension, but with this serious drawback, that, as there would be no sustaining means, as with steam boilers, to keep up the supply, the force within would diminish with every portion of the fluid drawn out. Every stroke of the piston acting on the wheels would lessen it, and not in a constant but a constantly increasing ratio. A car would not have one-third of the power it started with, to carry it over the latter half of its route.

Something better might be done than has perhaps been attempted or proposed:—the air should be in two separate receivers, one for the first half of the route, the other for the second. Three would be better than two. They should be kept at as low a temperature as circumstances permit while being charged. The cooler they are the more air they would take in. The introduction of fuel is surely to be avoided, but if a small quantity should offer a decided gain it would hardly be wise to reject it. For example: the motive fluid might be made to do something like double duty by discharging it from the working cylinder into a heated chamber, to act on a larger piston as in the caloric or common air engines, or by applying a moderate and moderately increasing heat to the receiver itself. In stopping to take in and let out passengers, cars waste entirely their momentum and strain the sinews of the horses to start again: the power thus lost might be saved by making an injecting air pump the drag instead of buffers on the wheels.

But with these additions the prospect of successfully employing air is not encouraging. There is another obstacle in the disproportionate dimensions of the receivers. Instead of being light and portable they would be the reverse. Suppose the engine of a car have a 4-inch cylinder with a 12-inch stroke—it could not well be less—its capacity would be 150 cubic inches. Sixty strokes a minute would use up $5\frac{1}{2}$ cubic feet in that time, and a 20 minutes route 109 feet. Now, a cylindrical receiver capable of withstanding the required pressure, say from 100 to 200 pounds on the inch—should not exceed 15 inches diameter. To contain the supply for 20 minutes it would have to be nearly 100 feet long; to work the engine half an hour 150 feet, and for a whole hour its dimensions would approach those of a car and be three or four times as heavy.

Perhaps this may be thought an exaggerated view; that the fluid ought not to be measured by the cylinder, like liquids drawn from hogsheads by the gallon, but that some credit should be given for expansion by cut offs, &c. To meet this it will be sufficient to observe that it has been calculated as operating on the piston with equal energy throughout and as being wholly used up.

Hence, so far from being, what many imagine it, a force that may be conveyed hither and thither in receptacles relatively no larger than soda fountains are to steam boilers, it is anything but that. Recourse must be had to explosive compounds for the only real portable force at present attainable. In these it is offered to us in the least of imaginable packages, and of startling intensities. Thus 12 grains of gunpowder exploded by Rumford in a $\frac{1}{4}$-inch cylinder drilled in wrought iron gave out a force, measured by a loaded valve on the open end, of 9·431 atmospheres. When the cavity was quite filled with 26 grains the force was calculated at 410·624 lbs., or 182 tons on the inch. Later experimentalists have reduced these wonderful statements. Robbins calculated the pressure of the gases generated by the explosion of powder at 1000 atmospheres, or somewhat less than $6\frac{1}{2}$ tons on the inch. But Hutton doubled this and Dr. Gregory confirmed his results. Others have since run it up to 2500 atmospheres.

To whatever cause the discrepancies are to be ascribed, to variations in the quantities of the powder, its filling or only partly filling the exploding chamber, &c., there is enough in all conscience in the lowest estimates to lead mechanicians to inquire if this amazing agent, resistless as an exploding force may not be converted into a quiet and efficient working one. Hautefille, Huygens, and Papin attempted this in the seventeenth century, and many more in the succeeding and the present one. Numerous notices may be found in the Mechanical Journals, but I do not remember any particular description of the testing apparatus, or any definite item of results. Desirous of ascertaining if the force could be introduced as a substitute for animals in rail cars at a moderate cost, I consulted my friend, Mr. James Bogardus, whose mechanical knowledge and inventive resource have rarely if ever been surpassed. The apparatus adopted was designed by him. It is only necessary to refer to a part of it here.

Instead of receiving the explosive impulse on air, the most elastic and durably elastic of agents, as suggested in the Patent-Office Report of 1849, the piston driven by the powder compressed a steel spring formed by bending to and fro, a bar, 20 feet long, 2 inches wide, and

¼-inch thick, into a series of short turns. The compressing force was determined directly by a weight passing over a pulley and making additions to it as the compression proceeded along a finely graduated scale. There could, therefore, be little or no mistake made in the amount of force thus received and given out to the revolving mechanism. The cylinder, or gun, was 2 inches bore and 16½ inches long. The piston and rod were of one piece of metal, the latter 1½ inches square.

The experiments were interrupted by the death of Mr. Dornbach, killed while packing a case of gun cotton. We had tried *white* powder and preferring it, were waiting a further supply from him. The delay in resuming them is moreover due to the fact that the magic power ascribed to powder in the books, and mining and military engineering, vanishes in the workshop.

The power of a single horse is commonly estimated at 33,000 lbs. raised one foot per minute.

We found 10 grains of powder only raised 250 lbs. one foot, hence 132 such charges, or 1320 grains, were required to bring it up to horse power. A pound contains 7000 and they could hardly keep the engine going 5⅓ minutes. Eleven pounds and a half would be consumed in an hour, and that at 20 cents per pound would amount to over twenty-two dollars for a working day of ten hours.

It should however be understood that from the construction of the testing machine, a feeding channel and a small receptacle for the residuum were exposed to the explosive impulse; and though the small quantity of air they contained gave back by expansion the compression received, there is little doubt that the effect on the piston would have been greater if there had been no communication with these—that is, if the force had been solely and strictly concentrated on the end of the piston. It was not intended to give any statements respecting the experiments till they were concluded, but on closing the remarks on Compressed Air, the opportunity seemed appropriate for a brief reference to them. E.

New York, October 18, 1864.

A Colorless Varnish.

From the London Chemical News, No. 246.

At the time the process of varnish-making by Luning was laid before the Society of Arts, Mr. Field put in a claim, when both the processes and products were found to answer the intended purpose, and the claimants were awarded twenty guineas each. Mr. Field describes his process as follows:—Six ounces of shellac, coarsely powdered, are to be dissolved by gentle heat in a pint of spirits of wine; to this is to be added a bleaching liquor made by dissolving carbonate of potash, and then impregnating it with chlorine gas till the solution becomes slightly colored. Of this bleaching liquor add one or two ounces of the spirituous solution of lac, and stir the whole well together. Effervescence takes place. When this ceases add more of the bleaching liquor, and thus proceed till the color of the mixture has become pale. A second bleaching liquor is now to be added, made by diluting muriatic acid with thrice its bulk of water, and dropping into it pulverized red lead till the last added portions do not become white. Of this acid bleaching liquor, small quantities at a time are to be added to the half-bleached lac solution, allowing the effervescence which takes place on each addition to cease before a fresh portion is poured in. This is to be continued until the lac now white separates from the liquor. The supernatant fluid is now to be poured away, the lac well washed in repeated waters, and finally wrung as dry as possible in a cloth. The lac obtained by the foregoing process is to be dissolved in a pint of alcohol, more or less, according to the required strength of the varnish; and, after standing for some time in a gentle heat, the clear liquor—which is the varnish—is to be poured off from the sediment. When the processes of Luning and Field came before the Society of Arts, the Editor of the *Franklin Journal** (Philadelphia) made known the process of Dr. Hare, in which he stated that "all the objects sought for were perfectly attained, and left nothing to desire, save on the score of economy." The following was Dr. Hare's process:—Dissolve in an iron kettle one part of pearl ash in about eight parts of water, add one part of seed or shellac, and heat the whole to ebullition. When the lac is dissolved, cool the solution, and impregnate it with chlorine till the lac is all precipitated. The precipitate is white, but its color is deepened by washing and consolidation. Dissolve in alcohol. Lac bleached by this process yields a varnish as free from color as any copal.—*British Journal of Photography.*

Preserving Timber from Decay.

From the London Builder, No. 1121

Experiments by Mr. Crepin are recorded in the *Annales des Travaux Publics de Belgique*, in connexion with Baltic timber creosoted by Mr. John Bethell's process. In October, 1857, creosoted fir, which had been taken from wood prepared in the ordinary way for the State railway, beech prepared with sulphate of copper, and fir and beech not prepared in any way, were placed 6 ft. 3 ins. (1·90) above the water mark upon the piles of the east pier at Ostend. In the beginning of January, 1859, the pieces were taken down and inspected. It was found that the piece of fir unprepared was much perforated by the teredo; that the piece of beech unprepared was perforated from one end to the other, that the pieces prepared with sulphate of copper were all three eaten by the teredo; and that the three pieces of creosoted fir alone were intact, and without a trace of the teredo. Mr. Crepin concludes that he cannot too strongly recommend the use of creosoted fir wood in hydraulic engineering, in preference to oak; since, in addition to its being cheaper, there is no doubt of the creosoted fir lasting longer. The Government Public Works Department has adopted this process, and constructed part of the dyke, and the whole of the American foot passengers bridge, in the new works at Ostend, of creosoted red fir. A report of Mr. Moir, with respect to the piles and other timber, in the pier at Leith Harbor, creosoted by Mr. Bethell, is equally satisfactory.

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* Journal of the Franklin Institute

Illumination of Lecture Rooms by Electric Light.

From the London Chemical News, No. 243.

The good and learned Abbé Moigno deserves the thanks of his numerous friends in this city for having introduced an innovation at his last lecture on the Progress of Science during the month, in the hall of the Societé d'Encouragement, which cannot fail to be adopted, in hot weather at least, by all *entrepreneurs* of public meetings or entertainments. Instead of being illuminated by the innumerable jets of gas with which the hall is provided, a single electric light, placed in a central position, lit the room in the most perfect manner. The consequence was that, although the thermometer was above 100° out of doors, no one was inconvenienced by the heat, and although there were nearly 500 persons present, hardly a single pocket handkerchief was used as a *sudarium* during the meeting. While enjoying the comparative coolness, one could not help thinking of the summer evening meetings of the London Chemical Society, held in the hottest and worst ventilated meeting-room it was ever my ill luck to enter.

The heat here during the last week has been positively frightful, and forms the only subject of conversation everywhere.

On some of the Strains of Ships. By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.

From the Lond. Civ. Eng. and Arch. Journal, Oct., 1864.

Read before the British Association.

In previous scientific investigations respecting the strains which ships have to bear, it has been usual to suppose the ship balanced on a point of rock, or supported at the ends on two rocks. The strains which would thus be produced are far more severe than any which have to be born by a ship afloat. The author computes the most severe straining action which can act on a ship afloat—viz: that which takes place when she is supported amidships on a wave-crest, and dry at the ends; and he finds that the bending action cannot exceed that due to the weight of the ship with a leverage of one-twentieth of her length, and that the racking action cannot exceed about $\frac{16}{100}$ths of her weight. Applying those results to two remarkably good examples of ships of 2680 tons displacement, one of iron and the other of wood, described by Mr. John Vernon in a paper read to the Institution of Mechanical Engineers, in 1863, he finds the following values of the greatest stress of different kinds exerted on the material of the ship:—

In the iron ship, tension	3·98	tons per square inch.
" " thrust	2·35	" "
" " racking stress	0·975	" "

It follows that in the iron ship the factor of safety against bending is between 5 and 6, agreeing exactly with the best practice of engineers; and that there is a great surplus of strength against racking.

In the wooden ship, tension 0·375 ton per square inch.
" " thrust 0·293 " "

Here the factor of safety is between 10 and 15, which is also agreeable to good practice in carpentry. As for the racking action, the iron diagonal bráces required by Lloyd's rules would be sufficient to bear one-third of it only, leaving the rest to be borne by the friction and adhesion at the seams of the planking.

Consumption of Water.

From the London Artizan, Sept., 1864.

A man is generally supposed to require about ½ gallon of water per day for drinking, cooking, &c., and about 4 gallons more for washing, bathing, and other purposes; a family of 5 heads will require about 9 gallons per day. In Paris, the consumption of this liquid is officially stated to be

4½	gallons	for every man per day.
16½	"	" horse "
9	"	for a two-wheeled carriage per day.
16½	"	four " " "
92	"	for every square yard of garden per annum.
66	"	for a bath per day.
⅛	"	for every square yard of public road per day.

The consumption in Madrid, according to the report of the directors of the Canal de Isabella II. Company, is

5¾	gallons	for every	man per diem.
21½	"	"	horse "
14½	"	"	two-wheeled carriage per diem.
21½	"	"	four " "
12	"	"	square yard of garden "

The following is the consumption in gallons, of water, per day and individual, in the chief towns of Europe and America:—Rome, 243; New York, 125; Marseilles, 103½; Besancon, 54; Dijon, 44; Bordeaux, 37½; Hamburg, 28; Genoa, 26½; Madrid, 26; Glasgow, 25; London, 24; Cette, 23; Lyons, 19; Manchester, 18½; Brussels, 17½; Monaco, 17; Toulouse, 16½; Geneva, 16¼; Narbonne, 16; Philadelphia, 15½; Paris, 15; Grenoble, 14½; Montpelier, 13; Nantes, 13; Voiron, 12; Clermont, 12; Edinburgh, 11; Havre 10; Angouleme, 9; Liverpool, 6; Metz, 5½; St. Etienne, 5½; Altona 5½; Constantinople, 4½; Rio de Janeiro, 2. This statement only comprises the quantities of water supplied by aqueducts; those yielded by wells and other means are not easy to ascertain.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, October 20th, 1864.

William Sellers, President, in the chair.

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

The Board of Managers and Standing Committees reported their minutes.

The Special Committees on Weights, Measures, and Coins, of the United States, and on a uniform system of Screw-threads and Bolt-heads, reported progress.

The following letter, accompanied by a box of shells, from Rear Admiral Dahlgren was read, and referred to the committee on Science and the Arts.

Flag Ship Philadelphia.
Charleston Roads, Sept. 12, 1864.

Franklin Institute, Philadelphia.

I transmit herewith the shells of some oysters just removed from the bottoms of iron clads in this squadron. They are the largest found there and are the growth of about six months; the whole bottom is entirely covered with oysters and grass to the great loss of speed which is thus reduced to some three knots the hour, and the rapid growth renders continued efforts necessary for its removal. There is reason to believe that zinc paint will prevent the formation of the oyster, but not of grass or barnacle. Still the removal of even these, detaches the paint also, more or less, and this opens the way for the oyster.

There is no obstacle so great as this, to the use of iron vessels, and I transmit these specimens, in the hope that the Institute may consider the subject worthy of its attention.

Very Respectfully,
J. A. Dahlgren, *Rear Admiral.*

Donations to the Library were received from the Society of Arts, London; Matthew B. Jackson, Esq., Sheffield, England; the Natural History Society of Montreal; and the Literary and Historical Society of Quebec, Canada; the American Institute of the city of New York, and the Managers of the State Lunatic Asylum Utica, New York; the Smithsonian Institution, and Frederick Emmerick, Washington, D. C.; and Prof. John F. Frazer, Philadelphia.

Mr. Nystrom made the following remarks on Dynamics:

It is my desire this evening to raise a discussion among the members of the Institute, on a subject which does not seem to be very generally understood, namely, on *Force*, *Power*, and *Vis-Viva*.

I have written an article on this subject for the *Journal of the Franklin Institute*, which article has been examined and rejected for publication by Professor Frazer. I have been told by the Actuary of the Institute that Professor Frazer says "there are so many errors in the article that it is not fit for publication."

About three weeks ago I had an interview with Professor Frazer, discussed with him the subject, and found his opinion to differ considerably from my own views, as well as from the general received opinion among philosophers of the present day.

I understood Professor Frazer to say that my "definition of force was wrong," and that "force is what I have defined as power," or "pressure multiplied by velocity."

I also understood Professor Frazer to defend Professor Tyndal's theory that heat is motion; he says that I am wrong in my remark on the same, and that I do not understand Professor Tyndal.

I have examined a great many works on the subject, but have not yet been able to find one which treats it clearly.

Moseley's work on Engineering, which is studied at the Military Academy, West Point, is very confused: it has one article on *Work*, one on *Moment of Inertia*, and one on *Vis-Viva*, all without examples or application. It is cruel to burthen a student with such mysteries as that book contain.

Foot-pounds of work and foot-pounds of power are often confounded with each other. In a late edition of Adcock's Pocket Book, a definition is given of units of work, after which the capability of a man working eight hours per day, is given in units of work, said to be copied from M. Morin, but the fact is that Morin's data are foot-pounds of *power* which Mr. Adcock has misrepresented by giving the capability of a man to be some 4000 foot-pounds of work per day, which is about two tons lifted only one foot in a day's work.

A man loading pig iron into a cart, lifts each pig about five feet high, and may walk a short distance with it, can load some fifty tons in a day's work, which is equivalent to at least 600,000 foot-pounds of work, instead of 4000, as given by Mr. Adcock.

The power of an ordinary man is about 50 pounds raised one foot per second, which will amount to between one and two millions of foot-pounds of work in a day.

One pound raised one foot is called one unit of work, by which the labor a man is capable to perform in a day, will be represented by millions. This unit of work is about the right size for a tumble-bug, for two millions of tumble-bugs can do about the same amount of labor as one man in a unit of time. That my comparison is not absurd I may mention that the ball which the tumble-bug rolls, weighs about a quarter of an ounce in this part of the country; but further South and in Asia, I have seen them as heavy as one ounce: two millions of those balls will weigh some 25 tons, lifted over the unevenness of the ground, I would roughly estimate to some 20 to 60 feet in one day, the work of which will be some 1500000 foot-pounds, or the same as that of a man.

You see the absurdity of applying such unit of work in practice, and the result is, that it is lost sight of, and used only by Professors in Colleges, and in school-books.

Here Mr. Nystrom was requested to give a definition of force, and explain the difference between *Force*, *Power*, and *Work*, to which he replied, I am yet not able to give a philosophical definition of force, and believe that that cannot be done until the physical constitution of heat, electricity and magnetism is revealed, but I have given in the article on Physical Work, a practical definition of force, which will hold good after a true philosophical definition is given.

The whole science of Dynamics, is contained in the following simple expressions.

Elements.	*Functions.*
Force $=$ F	Power $P = FV$.
Velocity $=$ V	Space $S = VT$.
Time $=$ T	Work $W = FVT$.

This includes also all cases of dynamics by the force of gravity, circular motion, and the whole confusion of *energy*, *moment of inertia*, *vis-viva*, &c. There are no more names necessary than the above six, namely, for the three elements and three functions. It will, however, be well to maintain the dynamical terms applied to moving masses, although they in reality mean the same as the above terms.

Mechanical terms.		*Terms for a moving mass.*
Force,	F.	Inertia.
Power,	$P = FV$.	Moment of Inertia.
Work,	$W = FVT$.	Energy.

The last term should be employed only for the change of motion of a moving mass.

F means the weight of a moving mass, which is denoted by M, in the articles on physical work.

The time $T = \frac{V}{g}$, from the law of gravitation,

and Work $W = \frac{MV^2}{g}$, energy.

In this formula it is supposed that the weight of the moving body is equal to its mass, which will hold good at the surface of the earth, where the acceleratrix $g = 32{,}166$.

The difference between weight and mass, is explained in the article on Work.

Force is recognised by the following terms:—Pressure, action, reaction, attraction, repulsion, compression, tendency, efforts, resistance, gravity, inertia, exertion, cohesion, electricity, magnetism, strain, stress, strength, thrust, burthen, load, squeeze, pull, push; all measured by weight. Force has also been improperly denominated, power, work, energy, throw, cast, heave, &c., &c.

Power is recognised by the following terms:—Dynamic momentum, moment of inertia, impetus, dynamic effect, momentum, traction, propulsion, impulsion, intensity; all measured by foot-pounds of power. Power has been improperly denominated, force, energy, work, quantity of motion, motion, inertia, strength.

Work is recognised by the following terms:—Energy, vis-viva, labor, throw, cast, haul, drag, draw, occupation, exercise, toiling, lift, raise, heave, cultivate, to tilt; all measured by foot pounds of work. Work has been improperly denominated, momentum of inertia, power, dynamic momentum, mode of motion, moment of activity, dynamic effect, quantity of action, &c., &c.

MOSELEY calls work "moment of inertia," but if inertia is force, moment of inertia must evidently be power. It appears that it would be more proper to call power "*Vis-Viva*," and work "*Moment of Vis-Viva.*"

In the estimation of foot-pounds of power I have made a deviation from Watt's rule. The unit 33000 pounds raised one foot per minute, I think is very unnatural. A velocity of only one foot per minute cannot be clearly conceived; it is only 0·2 or ⅕ of an inch per second: and on the other hand, the weight of 33000 pounds, or about 15 tons is too large, and very few can form a clear conception of its magnitude. A horse cannot lift direct a weight of 15 tons: the ordinary pull of a horse is 200 to 300 pounds, and a horse cart load is about one ton. Therefore it is my humble opinion that the foot-pounds of power ought to be brought nearer the ordinary peformance of a horse, or one pound lifted one foot per second as a foot-pound, which will be 550 foot-pounds per horse power. A velocity of one foot per second is conceivable, and 550 pounds can be lifted direct by a horse. This kind of foot-pounds is used in most parts of Europe. The Swedish horse-power is 600, the German 513 foot-pounds.

Lately there appeared in the *Journal*, a long article of inquiry on the subject of *Force*, *Power*, and *Vis-Viva*, written by Mr. Trautwine, which inquiry has not been answered. Mr. Trautwine is familiar with the leading scientific works of the day, and it is my humble opinion that, if he does not understand them, it is not his fault. We cannot take out of a book what is not in it.

Not long ago there appeared an article in the *Journal*, where foot-pounds were misapplied, which error renders that article worse than useless; for it may set readers of the *Journal* into confusion, and lead them into wrong speculations.

Numerous correspondents are seeking information on the subject through the *Scientific American*, but do not obtain satisfactory answers.

It is not necessary to dwell any longer on points where the subject is misconceived, for it is heretofore nowhere properly explained.

My article on Work is what I consider the subject in its true elementary form. Should like very much to have it criticised, improved, and simplified.

A Comparison of some of the Meteorological Phenomena of October, 1864, *with those of* October, 1863, *and of the same month for* fourteen *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W from Greenwich. By J. A. Kirkpatrick, A. M.

	October, 1864.	October, 1863.	October, for 14 years.
Thermometer—Highest—degree, .	75·00°	74·00°	90·00°
“ “ date, .	6th.	2d.	4th 1858.
“ Warmest day—Mean,	71·50	67·33	78·30
“ “ “ date,	6th.	2d.	6th 1861.
“ Lowest—degree, .	36·00	32·00	28·00
“ “ date, .	10th.	26th. & 27th.	25th 1856.
“ Coldest day—Mean,	44·83	40·00	35·80
“ “ “ date, .	22d.	26th.	27th 1859.
“ Mean daily oscillation,	13·16	16·10	15·57
“ “ “ range, .	4·56	4·93	5·43
“ Means at 7 A.M., .	50·11	49·79	51·19
“ “ 2 P.M., .	59.37	61·89	63·03
“ “ 9 P.M., .	53·18	54·18	55·34
“ “ for the month,	54·22	55·29	56.52
Barometer—Highest—Inches, .	30·022 in.	30·245 in.	30·452 in.
“ “ date, . .	3d.	26th.	25th 1861.
“ Greatest mean daily press.	30·015	30·209	30·378
“ “ date, . .	3d.	26th.	25th 1861.
“ Lowest—Inches, .	29·218	29·557	29·012
“ “ date, . .	28th.	2d.	26th 1857.
“ Least mean daily press.,	29·294	29·637	29·059
“ “ date, . .	28th.	3d.	26th 1857.
“ Mean daily range, .	0·129	0·108	0·142
“ Means at 7 A.M., .	29·715	29·920	29·915
“ “ 2 P.M., .	29·674	29·885	29·873
“ “ 9 P.M., .	29·726	29·925	29·899
“ “ for the month, .	29·705	29·910	29·896
Force of Vapor—Greatest—Inches,	0·703 in	0·648 in.	0·731 in.
“ “ “ date, .	6th.	2d.	7th 1861.
“ “ Least—Inches, .	·138	·094	·065
“ “ “ date, .	9th.	28th.	21st 1859.
“ “ Means at 7 A.M., .	·278	·303	·315
“ “ “ 2 P.M., .	·285	·306	·345
“ “ “ 9 P.M., .	·300	·320	·325
“ “ “ for the month,	·288	·310	·328
Relative Humidity—Greatest—per ct.,	91·0 per c,	95· per c.	97·0 per c.
“ “ “ date,	2d.	2d.	often.
“ “ Least—per ct.,	36·0	28·0	23·0
“ “ “ date, .	14th.	28th.	21st 1859.
“ “ Means at 7 A.M.,	72·7	78·9	78·2
“ “ “ 2 P.M.,	53·9	51·5	56·4
“ “ “ 9 P.M.,	70·6	71·7	73·3
“ “ “for the month	65·8	67·4	69·3
Clouds—Number of clear days,* .	12	9	9·6
“ “ cloudy days, ·	19	22	21·4
“ Means of sky cov'd at 7 A.M.,	54·8 per c.	60·3 per c.	56·6 per c.
“ “ “ “ 2 P.M.,	64·8	59·0	55·6
“ “ “ “ 9 P.M.,	34·5	40·6	40·0
“ “ “ for the month,	51·4	53·3	50·8
Rain.—Amount,	1·729 in.	2·663 in.	2·854 in.
No. of days on which Rain fell,	8.	10·	9·
Prevailing Winds—Times in 1000	N82°34′W·274	N61°23′W·153	N73°52′W.238

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* Sky one-third or less covered at the hours of observation.

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CIVIL ENGINEERING.

On Steam Boilers. By Zerah Colburn.

From the Lond. Civ. Eng. and Arch. Journal, Oct., 1864.

Read before the British Association.

The rate at which heat may be transmitted through an iron boiler plate, without injury to its substance, has never been precisely ascertained. About 70,000 units of heat per hour, equal to the evaporation in that time of one cubic foot of water from 60°, is believed to be the utmost square foot of plate of ordinary thickness. But, in order to approximately apply the *whole* heat of a furnace to the purposes of evaporation, a much larger area of heating surface, per unit of work done, is requisite. Watt fixed the proportion of one square yard of heating surface per cubic foot of water evaporated per hour, and this has been sanctioned by modern practice. But the average depth—or, in other words, the thickness of the stratum—of water thus boiled away is only $1\frac{1}{3}$d in. per hour, $\frac{1}{45}$th inch per minute or $\frac{1}{2700}$th inch per second, over the whole heating surface. From ten to twelve seconds are thus occupied in vaporizing a *couche* of water, no thicker than a single leaf of the paper upon which books are commonly printed. If, in proportion to the evaporation; an insufficient extent of heating surface be provided, there is not only a direct waste of heat—the products of combustion escaping at a temperature corresponding, perhaps, to that of incandescent iron—but the furnace plates may be burnt. Notwithstanding the active convection of heat in water, an intense flame, directed against the sides or roof of a boiler furnace, will, in time, crack or blister the iron. It is not certain that this result occurs from the inability of the metal to transmit the heat, for it is more likely that, under vigorous evaporation, the gravity of the liquid water (and it is the gravity only that brings it to the heating surfaces) is insufficient to bear down effectually against the rising volumes of steam. If, by powerful mechanical means, the water could be constantly maintained in contact with the heating surfaces, it is possible that the rate of evaporation upon a given area could be increased without injury to the plate. In the hardening of anvil faces and of steel dies, the requisite rapidity of cooling is obtained, not merely by immersing in water, but by its forcible descent in a strong jet upon the heated metal.

Under the conditions, however, of ordinary practice, no restriction of the heating surface is permissible. This surface is sometimes that of the exterior only of the boiler, but it is more usual, and on most accounts preferable, to dispose it internally by means of fire-boxes, flues, or tubes. The external surface of a boiler can only be increased by increasing its length or its diameter, or by increasing both of these together. Plain cylindrical boilers 90 ft., and in one or two instances 104 ft. in length, have been employed, but, even apart from any consideration of the great amount of space which they occupy, they are mechanically objectionable, and they are now no longer made. In increasing the external surface of a boiler by enlarging its diameter, it is weakened exactly in proportion to the increase, the bursting pressure, for a given thickness of plates, being inversely as the diameter. The danger attending the presence of a large quantity of heated water in a boiler is now well understood, and such as it is, it increases as the square of the diameter, so that, in a boiler of a given length, the elements of weakness and danger are collectively related to the cube of the diameter. External heating surface may be provided for in a number of smaller vessels, as in the retort boilers by Mr. Dunn, but these are of the water-tube family, which, heretofore, has been found subject to choking with the solid matter deposited by the water.

Next are the boilers with internal heating surfaces. Internal fire tubes were in use in steam boilers in the last century, and they were applied within a cylindrical barrel by the Cornish engineers, among whom was Trevithick, who employed both the straight flue and the return flue, and who made the fire-place within the flue. The Cornish boiler, in this form, was improved by Mr, Fairbairn and the late Mr. Hetherington, who added another fire tube, thus making the two-flued boiler now so extensively employed in Lancashire. The two flues, although somewhat smaller than the single flue, afforded a greater extent of heating surface, besides securing increased regularity in firing. The principle of subdividing the flame and heated products of combustion, so as to obtain greatly increased heating surface within a barrel of given diameter, was fully carried out in the multitubular boiler invented by Neville, of London, in 1826, employed by Seguin, in France, in 1828, and subsequently in the Liverpool and Manchester locomotives, from which it has been handed down to the present practice of engineers. Not since Watt's time, however, has the evaporative power of a square foot of heating surface been increased, the improvement in the plan of steam boilers being that, chiefly, of inclosing a greater extent of surface within a given space. The heating surface in the boilers of the *Great Eastern* steamship is equal to the entire area of her vast main deck; that in the *Adriatic* measures more than ¾ of an acre; while the *Warrior* and the *Black Prince* have in their boilers 2500 square yards of surface of tubes, the aggregate length of which is more than 5½ miles. But it is only where, as in steamships and locomotive engines, the dimensions and weight of boilers must be the least possible, that the multitubular arrangement is even to be tolerated. It is costly, and subject to rapid decay. In steamships especially, the life of multitubular boilers is comparatively short. The boilers in her Majesty's vessels of war are found to last but from five to seven years; those of the West Indian Royal Mail ships last, according to Mr. Pitcher, of Northfleet, six years only; and those of the Dover and Calais packets taking the testimony of the former mail contractor, Mr. Churchward, need to be renewed every three and a half or four years. On land, multitubular boilers working under a constant strain, and in most cases, as constantly concentrating a saturated solution of sulphate of lime, are nearly out of the question for the purposes of manufactories, although there are instances of their employment even in spinning-mills.

A boiler rated at 40 nominal horse power will ordinarily evaporate 60 cubic feet of water per hour, or upwards of 100 tons of water per week of 60 hours. And feed water, containing as much as 40 grains of solid matter per gallon, is often regarded as very good, not only when the inorganic impurity consists of the deliquescent salts of soda, but even when it is neither more nor less than an obdurate carbonate or sulphate of lime. Whatever the solid matter contained in the water may be, it is never carried over with the steam, but is left behind in the evaporating apparatus, and 100 tons of water, fed in a single week to a boiler in the manufacturing districts commonly contains a hundred-weight or more of dissolved gypsum or marble, and of which all that is not held in solution is deposited in a calcareous lining upon the internal metallic surfaces. This fact will explain why not only water tube, but multi-fire tube boilers cannot be economically employed under the ordinary circumstances of steam generation. The consideration of deposit or scaling, as well as that of workmanship, imposes a limit to the subdivision of heating surface amongst a great number of small tubes. In ordinary boiler making the geometrical advantage of the sphere cannot be turned to account. It cannot be produced economically in plate-iron, nor, if made in plate-iron, could it be advantageously applied in a steam boiler. The hollow sphere has this property, to wit: with a given thickness of metal it has twice the strength of a hollow cylinder of the same diameter. This is upon the assumption (which is correct where the cylinder is of a length greater than its own diameter) that the ends of the cylinder offer no resistance to a bursting pressure exerted against the circumference. Under over-pressure, a closed cylinder would take the shape of a barrel, and if of homogeneous material and structure it would burst at the middle of its length, and in the direction of the circumference. The circumference of a sphere of a diameter of 1 being always 3·14159, the sum of the length of the two sides of a cylinder of the same diameter, and having a plane of rupture of the same area, is 1·5708, or exactly half as much. And not only are the boiler-heads of no service in resisting the strain in the direction of the circumference of the cylinder, and not only are they weak in themselves, except when of a hemispherical form, or when well stayed, but furthermore, the whole pressure against them is exerted to produce a strain of the sides of the cylinder in the direction of its length, and where there are no through stay-rods between the opposite heads, this strain is necessarily equal to one-half of that exerted in the direction of the circumference.

The bursting pressure of steam boilers is commonly calculated from the average tensile strength of wrought iron plates. This strength is very variable, however, and it would be more logical to take the minimum. The most extensive series of experiments upon the strength of iron plates is that made by Mr. Kirkaldy for Messrs. Napier, of Glasgow. The number of samples of each description of iron tested was not large yet the tensile strength ranged between very wide limits. That of Yorkshire iron varied between 62,544 ℔s per square inch and 40,541 ℔s, both specimens being from the same makers. Staffordshire plates varied between 60,985 ℔s, and 35,007 ℔s, and Lanarkshire plates between 57,659 ℔s and 32,450 ℔s. The conclusion cannot be resisted that engineers are frequently dealing with boiler plates of a tensile strength not greater than from 16 tons to 18 tons per square inch, notwithstanding that the average strength may be 22 tons, and the maximum 27 tons. And the loss of this strength in punching the rivet holes is not merely that of the iron cut out, but the punch is found to sensibly injure that which remains. Mr. Fairbairn's well known and frequently verified ratio of 56 to 100, as the strength of a single riveted joint to that of the unpunched plate, must be always admitted in calculations of the strength of riveted boilers. The 40-horse Lancashire boiler, 7 ft. in diameter, will thus be often found to have an ultimate strength not greater, when new, than that corresponding to a pressure of from 210 ℔s to 235 ℔s per square inch. This, however, is without taking account of the strain exerted longitudinally upon the shell of the boiler by the pressure on the ends, and it is upon the assumption, which is hardly tenable, that the boiler heads, and especially the flues, are of the same strength as the cylindrical body or shell. Without the angle iron strengthening recommended by Mr. Fairbairn, the collapsing pressure of the flues of large boilers was found, in that gentleman's experiments, to be sometimes as little as 87 ℔s per square inch. The strain resulting from the circumferential and longitudinal components, in the outer shell, is one-eighth greater than that calculable for the circumference alone, so that, even if the heads and flues were stayed to the strength of the shell, this would correspond to a pressure of but from 190 ℔s to 210 ℔s instead of 210 ℔s to 225 ℔s, as just supposed. But these estimates are for the strength of the boiler when new. In the experience of the officers of the Manchester Boiler Association, with from 1300 to 1600 boilers always under their care, one boiler out of every seven, and, in some years, as in 1862, nearly one of every four, became defective by corrosion alone, while of every eight boilers examined in the course of a year, seven are found to be defective in some respects. Thus, in 1862, with 1376 boilers under inspection, 85 positively dangerous, and 987 objectionable defects, were discovered, 37 dangerous and 270 objectionable cases of corrosion alone having been reported. As a boiler malady, corrosion corresponds in its comparative frequency and fatality to the great destroyer of human life—consumption. It is the one great disease. It is frequently internal in consequence of the presence of acid in the water; but it is still oftener external, and it is most insidious and certain wherever there is the least leakage of steam into the brick-work setting. Condensed steam, or distilled water is an active solvent of iron, as well as of lead; and peaty water, which, so far as *inorganic* matter is concerned, is very pure, and distilled water from surface condensers, and indeed any water that is quite soft, is known to eat rapidly into the substance of the boiler in which it is used. A trickling, however slight, of condensed steam, down the outside of a boiler, will infallibly cause corrosion, and to this was directly traced a large number of the forty-seven boiler explosions which occurred in the United Kingdom, in 1863, and which were attended with the loss of seventy-six lives, besides injuries more or less serious to eighty persons.

Corrosion is most rapid where the iron is comparatively pure, as in the best Yorkshire and Staffordshire plates. The presence, however, of a small proportion of carbon, as in steel, or especially of silica and carbon, as in cast iron, renders it nearly indestructible. The experience even with cooking utensils demonstrates this, but it is more satisfactory to observe the fact in engineering operations on the great scale. When Nelson first introduced the hot blast, he employed wrought iron heating stoves, and although only 300° Fahr. was at first fixed upon for the temperature of the blast, the stoves were rapidly destroyed. It need hardly be mentioned that a wrought iron gas retort would be worthless, where cast iron answers well, being inferior only to fire-clay. It is the same with forge tuyeres, cast iron lasting indefinitely. Since superheated steam began to be generally employed, much difficulty has been experienced from the rapid corrosion of the superheaters. The Peninsular and Oriental Company's engineers have been compelled to adopt copper, instead of plate iron, heating surfaces for this purpose. Messrs. Richardson and Sons, of Hartlepool, have on the other hand adopted cast iron, and their superheaters of this material show no corrosion whatever after four years use. The sulphurous fumes from locomotive engines rapidly corrode the plate iron station roofs, while the cast iron girders and cornices remain unaffected. Cast iron bridges are indestructible by rusting, while large quantities of iron scales are being removed from wrought iron bridges, including the Conway and Britannia tubes. From abundant experience with cast iron steam boilers and the tubes of cast iron heating apparatus, it may be taken as settled that, where the thickness is moderate, cast iron may be thus employed without the possibility of corrosion.

The tensile strength of cast iron varies between 5 tons and 15 tons per square inch. Considered as a material for boilers only the minimum strength should be regarded, exactly as from 16 tons to 18 tons has been taken for wrought iron plates. Cast iron boilers 8 feet in diameter, and of great length, were at one time made, but these were manifestly objectionable. The spherical form, of moderate diameter, is preferable, and whatever is the bursting strength of a riveted wrought iron cylinder, that of a cast iron sphere of the same diameter and the same thickness of metal will be the same. The plate iron of a strength of 18 tons per square inch, is virtually weakened to 10 tons by the loss in riveting, and as the hollow sphere is twice stronger than the hollow cylinder of the same diameter and thickness, the cast iron, having no joints, becomes equal in this comparison, to the wrought plate. If we could always count upon the maximum strength of iron, to wit, 27 tons per square inch for wrought, and 15 tons for cast, a 14-foot cast iron sphere would have the same strength to resist bursting as the 7-foot cylinder of the Lancashire 40-horse boiler, supposing the same thickness of metal in each case. But there is no occasion to make a boiler a single large sphere, for it is now ascertained from extensive experience that hollow cast iron spheres of small diameter do not retain the solid matter deposited by the water. Small water tubes, and, indeed, all small water spaces in ordinary boilers, always choke with deposit when the feed water contains lime, but cast iron boiler spheres although they may be temporarily coated internally with scale, are found to part with this whenever they are emptied of water. This fact is the most striking discovery that has been made in boiler engineering. It removes the fatal defect of small subdivided water spaces, which can now be employed with the certainty of their remaining constantly clear of deposit. This discovery has been made in the use of the cast iron boiler invented by Mr. Harrison, of Philadelphia, U. S., and which is now working in several of the Midland and Northern counties. Mr. Harrison employs any required number of cast iron hollow spheres, 8 inches in external diameter, and $\frac{3}{8}$-inch thick, these communicating with each other through open necks, and being held together by internal tie-bolts. A number of these spheres are arranged in the form of a rectangular slab, and several of these slabs, set side by side and connected together, form the boiler, about two-thirds of the whole number of spheres being filled with water, while the remainder serve as steam room. The bursting strength of these spheres corresponds to a pressure of upwards of 1500 lbs per square inch, as verified by repeated experiment, being, therefore, from six to seven times greater than that of the ordinary Lancashire boilers of large size. The evaporative power, as in all other boilers, depends upon the extent and ratio of the grate area and heating surface, but in practice from $7\frac{1}{2}$ lbs to 8 lbs of water are evaporated per pound of coal in a cast iron boiler, which, for each ton of its own weight, supplies steam equal to 10 indicated horse power. The joints between the spheres are made by special machinery, securing the utmost accuracy of fitting, and there is no leakage, either of water or steam. The spheres occupied as steam space are screened by fire-bricks from the direct action of the heat, but enough is allowed to reach them to secure complete drying, and, if desired, any degree of superheating of the steam. The slabs, into which each series of spheres is assembled, are placed in an inclined position, which secures the thorough circulation of the water. The whole quantity of water carried in a 40-horse boiler is 3 tons, the boiler weighing 13 tons, and presenting 1000 square feet of water heating, and 500 square feet of steam drying surface. In Manchester, with the feed water taken from the Irwell, or from the canal, a hard scale is soon formed in the ordinary boilers; but in the cast iron boiler a succession of thin scales of extreme hardness are found to form upon, and to become detached of themselves from the inner surfaces of the water spheres. These scales are blown out with the water at the end of the week, and only small quantities can be discovered when purposely sought for. A specimen of these, slightly cohered together into a friable mass, is exhibited. A pint of loose scales and dirt is the most that has yet been found on a careful internal examination after nine months daily work. None of the iron is removed with the scale; the weight of the spheres, after three years service, being the same as when new. In America, Mr. Harrison's cast iron boiler has been worked for six years. Messrs. Denton, chemists, of Bow-common, London, have had one in use for three years, and for the last two years the same description of boilers have been employed at Messrs. Hetherington's and other large works in Manchester. It should be added that the system of casting the spheres is such that their thickness is necessarily the same at all points.

The self-scaling action, which has been found to be the same in all cases where the boiler has been worked, can only be explained by conjectures, which it is not, perhaps, necessary to introduce into the present paper. It deserves the careful investigation of the chemist and mechanical philosopher, with whom the author prefers to leave the subject.

Recent Experiments on Artificial Stone, &c.

From the Lond. Civil Eng. and Arch. Jour., August, 1864.

The following is extracted from the report of the committee appointed by the council of the Royal Institute of British Architects, 2d March, 1863, for the purpose of making experiments on artificial stone.

This Committee met on the 11th of March, 1863, and at once issued notices, and advertised in the public journals, that it was desirous of receiving prospectuses or other information connected with the manufacture of Artificial Stone; and resolved—

> That each patentee be informed that this Committee proposes to report merely on the actual facts which may appear in the investigation;

That, so far as possible, the various materials used in the investigations be supplied under the immediate supervision of this Committee;

That Mr. C. H. Smith, Honorary Member, be added to this Committee;

That some eminent Chemist be associated with this Committee, in order to assist it in the investigation of the processes [Mr. Alfred White was thereon added]; and

That each patentee be requested to inform this Committee of the proceedings he would propose to adopt with regard to the investigation of his process.

It received responses from Messrs. Coignet, Ransome, Bodmer,* and Wheeble.†

Mr. Charles M. Westmacott put forward his patent cement and plaster; and they, although perhaps beyond the scope of the Committee, were received by it, on account of the interest that had been shown in the patent at the meeting of the Institute on the 23d of February, 1863.

The Committee met eleven times at Mr. Dines's, Grosvenor-road, twice at Mr. Tracey's, and once at Mr. Ransome's, in Cannon-row, exclusive of a visit to his works at Ipswich on the 15th of December last.

On the first meeting at Mr. Dines's, the Committee met Mons. Coignet for two successive days, (7th and 8th of April, 1863,) who prepared before it various blocks of his patent beton, which were experimented upon at future meetings. The Committee having had regard to the state of materials after exposure to atmospheric influences, and the reverse, the results in these as in all its other experiments may be taken as certified by the extreme care that it has taken, and are herewith appended in tabular forms.

The Committee met in Cannon-row on the 15th of May, 1863, and Mr. Ransome thoroughly explained, in the most open and satisfactory manner, his process; but as he stated that he could not, on so small a scale of manufacture, and without machinery, provide fair samples, it permitted him to prepare (from moulds supplied) samples at his works at Ipswich, and to forward them to London, guaranteed by him, to be experimented upon before it. The Committee, however, not being satisfied with the results, thought it advisable to see samples made, and intimated its wish to Mr. Ransome, who most liberally received it at Ipswich on the 15th of December last, and exhibited his mode of manufacture on a large scale. Specimens from the moulds supplied by the Committee were made before it, which were tested in like manner.

The Committee deemed it advisable, for the better understanding of the results obtained, to institute experiments upon other materials as a means of comparison, the results of which are appended in Tables D, E, and F; and it would particularly direct attention to Table D, showing a series of experiments suggested by Mr. John W. Papworth, on cubes of Portland stone, 2″, 4″, and 6″ in height, by their respective beds, cut from a carefully selected block 2′ cube. The deductions to be drawn from this table appear to be, that the results obtained from crushing small cubes of 2″ and under on the side, as usually given in accepted tables, must be received with considerable caution, inasmuch as while 2″ Prisms 2″ × 2″ bore 1·5 of a ton put on the square inch, and 2″ Prisms 4″ × 4′, and 2″ Prisms 6″ × 6″, nearly 2·5 tons, yet 6″ Prisms 6″ × 6″ and 4″ Prisms 4″ × 4″, only bore 1·9 of a ton on the square inch, showing that the strength certainly increased with the size of the area, but apparently not in any definite proportion; while it was observed in these experiments that the height had considerable influence upon the ultimate resistance, a result not expected, and in contradiction of the usual neglect of the height, until that element exceeds six times the diameter.

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* Patent Stone Bricks.

† Reading Abbey Stone.

From Table F, it appears that the cohesive power of cements may be classed in the following order:—Martin's, Portland, and Roman; but it must be borne in mind that none of these had been exposed to atmospheric influences; yet the very remarkable differences without any appreciable cause in the results of the various experiments shows that the strength of cements is a subject upon which no satisfactory theory has yet been definitely propounded.

The Committee trusts that the time required for preparing and testing, at given intervals, so many and various materials, will be sufficient reason for not laying the Report earlier before you; and it will be seen from the extreme variation in the results given by the experiments, that the Committee is justified in repeating the experience of all zealous investigators—viz: that, without regard to the well-known inapplicability of general formulas to particular works, the council must not be surprised at finding that the Committee will not draw peremptory deductions from even the experiments made, considering that the least number, to arrive at anything like a perfect conclusion, should be more than twenty of each size of every material: the Committee felt that it had no right to exact so much time and expense as thereby would be required from the professional gentlemen of your Committee, or from Mr. Dines, who personally superintended the details of each experiment; and, moreover, it had no funds placed in its hands by the Council for that purpose.

The Committee feels anxious to express its sense of the liberal and zealous manner in which Mr. Dines put the resources of his establishment, and his own time at its disposal, furnished the samples and materials from which most of these experiments were made, and suggested the form which it believes to be quite novel of moulds for those to prove by tensile strength the cohesive power. The Committee otherwise could not possibly have carried out the experiments to the extent which, however incomplete, has furnished, it believes, as far as they go, a very trustworthy parallel of the strength of materials, deduced from specimens of a larger size—viz: 4″ cubes—than has generally been adopted.

Table showing Strength under Pressure of different Materials.

4-INCH CUBES.

MATERIALS.	Cracked.	Crushed.	on sq. inch.	REMARKS.
STONE.				
Portland.	Tons.	Tons.	Tons.	
Brown Top Bed.	. .	26·7	1·66	average at 1·11 went at once :
" "	19·8	23 0	1·43	at ∟ to bed.
" "	17·8	19·0	1·18	at ∟ to bed.
" "	16 7	17·5	1·09	
" "	14·1	16 2	1·12	at ∟ to bed.
" "	. .	15 8	0·99	went at once shelly.
" "	7·5	12·2	0·76	split both horz. & perp
" "	10·6	11·5	0·70	
Base or Best Bed.	32·6	36·2	2·25	average at 1·42 split horizontally.
" "	13·5	28·0	1·75	
" "	26·4	27 0	1·68	
" "	18·0	20·1	1·25	on bed.
" "	16·4	16·5	1·31	"
" "	11·4	14·4	0·90	∟ to bed.
" "	13·3	13·4	0·83	"
Mansfield.				
Red	39·0 *a*	40·1	2·5	*a* at 28 0
"	49·7	68·8	4·3	scaled off in small pieces. both very fetid.
White	21·5	36·4	2·27	scaled at 21·5
"	12·0	24·0	1·5	
Forest of Dean.				
White	17·5 †	47·8	2·98	† slight crack external only
"	43·0	45·0 *	2·81	* not considered very best.
"	30·0	40·0	2 5	all broke suddenly like iron
"	38·0	39·4	2·46	
Bath.				
Corsham ..	8·0	8 84	·55	
" ..	. .	8 3	·51	crushed suddenly.
" ..	7·5	8·1	·51	
Box Best Bed.	. .	5·4	·32	
" Ground, "	. .	8·6	·53	broke at once into 4 cones
" " " ..	5·7	5·85	·36	
" " " ..	. .	4·35	·32	
Caen.	8 2	15·4	·93	
.. ..	7·5	12·7	·79	
Gypsum.				
Derbysh. Alabaster,	10·9	11·0	·72	
Marble.				
Cornwall Serpentine				
" Poltesco Green	25·0	52 0	. .	at 15·4 slightly, at 40· going fast, smashed.
" Signal Staff Blk. & Rd.	10·0	51·0	3·2	smashed.
" " "	10 50	37·75	2 35	

Table Continued.—4-INCH CUBES.

MATERIALS.	Cracked.	Crushed.	on sq. inch.	REMARKS.
SLATE.	Tons.	Tons.	Tons.	
North Wales.				
Machynlleth . .	. .	83·5 *a*	5·21	*a* at 71 splinter flew off
Bangor (Penryhn)	. .	80·0 *b*	5·0	*b* no effect, neither b'k
Portmadoc—				
Welsh Slate Com.	20·6	75·3	4·70	
Festiniog ditto.	20·0	70·5	4·40	
. . . . . .	. .	63·0	3·93	
Macnoffern	23·6	65·4	4·08	
Pen-Machno, Conway.	. .	48·8	3·05	4·80 average.
Dinorwic, Lllanberis	. .	49·0	3·06	
Cornwall.				
Delabole . .	52·0	61.6	3·85	broke at ∟ to bed.
Ireland.				
Valencia.	. .	57·3	3·58	
WOOD.				
Teak,, Moulmein . .	. .	50·8	3·17	crushed endways.
Deal, Archangel . .	. .	28·8	1·11	" fibres torn apart.
CONCRETE.				
1 Lime 3 Ballast	. .	5·1	·31	3 weeks old.
1 Slk. Lime 3 ballast	. .	5·2	·32	
MARTIN'S CEMENT.	. .	14·85	·92	22 hours old.
PLASTER, Common.	. .	4·95	·30	22 hours old.

9-INCH BRICKS.

MATERIALS.	Cracked.	Crushed.	on sq. inch	REMARKS.
	Tons.	Tons.	Tons.	
Common Stocks . .	. .	30·0	·7407	
" " . .	. .	18·0	·4432	
" " . .	. .	12·0	·2962	
" ground sides	22·5	26·8	·6616	
" " "	. .	31·5	·7777	
Burham Brick . .	59·8	73·6	1·817	
" " . .	. .	80·0*a*	1·985	*a* Not crushed.
" " . .	. .	62·0*b*	1·310	*b* Cracked slightly, not crushed.
Place, bad . .	8·4	9·0	·2222	
"	8·0	10·7	·2641	
Grizzle	. .	14·0	·3456	

Table Continued.—9-INCH BRICKS.

MATERIALS.	Cracked.	Crushed.	on sq. inch.	REMARKS.
Stocks.				
2 joined in Mortar	5·0	8·7	·1071	
† " " in Cement	7·0*	15·35*	·1896	*Small pieces flew off at intervals, but cement stood.
Place.				
2 joined in Mortar	. .	5·3	·0641	
† " " in Cement	. .	9·4	·1160	†About 14 days old.

SHAFTS, 12 inches long, 3 inches diameter.

MATERIALS.	Cracked.	Crushed.	on sq. inch.	REMARKS.
STONE.	Tons.	Tons.	Tons.	*All yielded vertically.*
Portland, worked · ·	7·3	10·25	1·48	bedded in leather.
" rgh. tooled	. .	8·57	1·00	" in plaster.
Marbles (Devonshire.)				
Ipppleen, Mottled red,	9·2	10·7	1·37	
Poltesco, grey-green,	4·3	4·3	·60	went across and not with vein
" "	. .	6·0	·84	went at once.
Signal-Staff, red & blk,	. .	33·5	4·73	went in fragments.
" "	20 0	22·5	3 18	" "
" "	12·75	16·25	2·29	
Cadgewith, grn. & blk	16·92	17·62	2·49	
WOOD.				
Deal, Archangel · ·	. .	19·1*a*	2·70	*a* lost 3-16ths in length: point of yielding 5·12ths of length in both.
6″ of above sawn off left	. .	19·3	2·73	
Deal, Archangel · ·	. .	16·3*a*	2·30	
6″ of above sawn off left	. .	19·4	2·74	
Teak, Moulmein · ·	. .	18 25*b*	2 58	*b* lost 1-8th and point of yielding about same.
6″ of above sawn off left	. .	18·7	2·64	
Teak, Moulmein · ·	. .	16·0*b*	2 26	
6″ of above sawn off left	. .	17·75	2·50	

The Committee then proceed to state, that having regard to the difficulty of drawing inferences from a long series of tables, and the desirability of general statements of results, they have, at considerable labor to themselves, prepared such statements. Of these the following are the chief:—

Mons. Coignet's process is admitted to be highly successful on a large scale in Paris; but the majority of the cubes prepared by him do not appear to be much stronger under pressure than ordinary concrete, and inferior to Bath stone, although they improve by age; and this weakness is impugned by him as a result on the ground that the samples were made out of his factory, and that he had not all the appliances requisite at command. A step-landing, and bas-relief, made, in April, 1863, from the sweepings of refuse used for making blocks for experiments, are still at Mr. Dines's, and having been exposed to the weather, as well as to wear and tear, appear to stand as well as Portland cement.

Mr. Ransome's process, the Committee states, appears to be founded on scientific principles, and theoretically ought to answer. It was, however, found that there must be some difficulty in insuring the complete combination of the ingredients, and that few of the specimens especially prepared for the Committee were thoroughly successful in that respect, and but few of those made in its presence. There is an important difference between these and Mons. Coignet's examples, that Mr. Ransome is not able to declare that his process is highly successful on a large scale, but represents that he has not yet completed the appliances to put it into operation. In some of his specimens there appears an improvement as they advance in age; but so irregular is this that no reliance can be placed upon that feature. But it would seem that, if the process of penetration is complete, the ultimate strength is attained in a short period, and that the external influences of weather had little effect upon them in that respect. The Committee feels that if Mr. Ransome can insure the perfection of his entire process, restrict himself to the production of work for each occasion, as it may arise, and (for the sake of artistic excellence) avoid becoming a manufacturer of casts from a certain number of moulds, it is possible that for external decorative purposes there is a large field open to him; but they are of opinion that, as yet, Mr. Ransome has not rendered his material suitable for the chisel of the carver or the tool of the modeler.

Mr. Wheeble (Reading Abbey Concrete Stone) sent moulded bricks, made at Reading, from the works to the Committee, and they seem to have an ultimate strength, apparently produced by the use of stone lime from Bridgewater, equal to that of common stocks; the latter however possess the advantage of not yielding until the instant of disintegration. The 4″ cubes made in moulds supplied by the Committee, and in its presence, never attained the strength of concrete except in a case where large gravel or flint was the chief ingredient.

Messrs. Bodmer (Patent Compressed Stone Bricks) expressed their inability to comply with the requisitions of the Committee as to 4″ cubes, as the moulds of their press were of ordinary brick size; and if their material were pressed by manual labor into 4″ cube moulds it would not represent the artificial stone which they were in the habit of producing. The Committee accepted accordingly bricks supplied from their works, which show a steady progressive increase in strength as they advance in age; and the results justify the statement made by Messrs. Bodmer, that after six weeks to two months the bricks are fit for use. The Committee regrets the unfortunate inability of Mr. Charles Westmacott to put himself satisfactorily in connexion with persons having at their disposal the premises and materials which he required for the display of the qualities of his processes. These chiefly depend upon the mixture of natural materials before adding water. The Committee has been able, however to test one or two experiments, which result in giving it a favorable opinion of the system adopted by him both for ceilings and for composition applied internally or externally. These may still be seen in Mr. Dines's yard, having been exposed through last autumn and winter. The quick stucco, for rendering ceilings, was tried in the month of May, 1863, consisting of unslacked lime, chalk, and sand, mixed pure, before adding water; it occupied in making and setting twenty minutes, was followed within an hour by ordinary gauged stuff, and stood without cracking. Messrs. Francis, of Nine Elms, have recently made arrangements with Mr. Westmacott to apply his processes practically; from their operations, on a more extended scale than was possible to be carried on before the Committee, the valuable character of his invention may be more decidedly proved.

Had the results of the experiments now laid before you been more consistent with each other, and had they resulted in conclusions highly favorable to the various processes, the Committee would still have felt bound to impress upon the members of the profession who have the opportunity of trying novelties, that future experiments for testing the strength of materials should be made on cubes of larger dimensions than have been heretofore employed; and they would suggest that cubes of 6″ sides would perhaps be the simplest and best adapted for the purpose: with regard to time, years instead of months are necessary to enable any one to arrive at practical conclusions as to the durability of any artificial material put forth in lieu of natural ones.

From among the tables appended we select the two given, in preference to those which tabulate the results of experiments on artificial materials, principally on the ground that experiments with the latter must be regarded as not yet furnishing results of permanent value, owing to the constant changes and improvements still being introduced.

Table showing the results under pressure of Cubes 2″, 4″, and 6″ in height, by their Base.

PORTLAND STONE (*Brown Bed.*)

HEIGHT.	Base.	Cracked.	Crushed.	on sq. inch.	REMARKS.
	″ ″	Tons.	Tons.	Tons.	
2″	2 × 2	. .	3·2	0·8	At once.
"	2 × 2	4·0	6·0	1·5	
"	4 × 2	. .	20·2	2·5	At once.
"	2 × 6	21·0	23·5	1·70	Across the bed.
"	4 × 4	8·0	41·0	2·5	
"	6 × 6	64·0	86·0	2·4	
4″	2 × 2	. .	3·0	0·75	
"	4 × 2	. .	17·0	2·12	
"	4 × 4	25·75	29·25	1·82	
"	4 × 4	24·25	28·75	1·85	
"	4 × 6	31·0	45·0	1·87	*a* Very slight
"	6 × 6	48·0*a*	82·0*b*	2·27	external.
					b Not crushed.
6″	2 × 2	2·8	3·4	0·85	
"	6 × 2	. .	10·0	0·83	
"	4 × 4	18·0	20·45	1·27	∟ to bed.
"	6 × 4	28·0	32·0	1·33	
"	6 × 6	64·0*c*	70·0	1·94	*c* Very slight.
"	6 × 6	55·0	68.75	1·90	

For the Journal of the Franklin Institute.

General Problem of Trussed Girders. By De Volson Wood, Prof. of C. E., University of Michigan.

(Continued from page 311.)

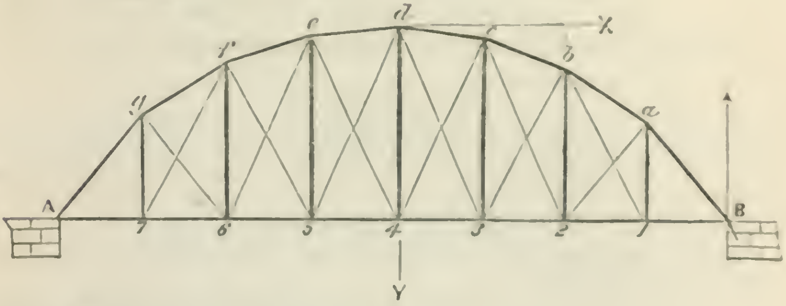
The formulas, which express the strains upon the several parts of trusses which are in common use, should be as simple as possible, and when possible should be continuous functions of the strain and distance from some fixed point, as one end for instance. This can generally be done when the curve of the chord follows an algebraic law.

We will suppose that the bays on the lower chord are all equal to each other, and that the load consists of equal weights placed at the upper or lower joints. If these hypotheses do not correspond with any practical case resort may be had to the equations already given.

14°. *Let the lower chord be horizontal and the upper one parabolic, or, if polygonal, let the vertices of the polygon be in a parabolic arc.*

Some parts of this problem were very fully discussed in my article on the "*Trussed Arch,*" found in the April number of this *Journal* for this year, on page 223. I shall therefore notice it very briefly here, merely showing how to apply equations (20) to this case.

FIG 11.



Let Fig. 11, represent the truss.

Let N = total number of bays in the horizontal tie,

n = the number of the bay considered, counting from either end,

D = $d4$ = greatest depth of the truss,

l = length of a bay = 1 — 2 = 2 — 3, &c.,

p = one of the equal weights which constitute the load,

W = the total load when uniformly distributed over the span; and the other notation the same as before given.

Then $p = \frac{W}{N}$.

We first suppose that the bridge is uniformly loaded throughout with equal weights placed at the joints 1, 2, 3, &c. Then we readily find $V = \frac{1}{2}(N-1)p$, $\Sigma_0^x P = (n-1)p$; $x_1 = nl$ . . (26)

and $h = \frac{4D}{N^2}(N-n)n$, $\Sigma_0^x Px = \frac{1}{2}n(n-1)pl$.

These substituted in the third of equations (20), and reduced will give

$$H_1 = \frac{plN^2}{8D} \qquad . \qquad . \qquad . \qquad . \qquad (27)$$

Equation (27) being independent of n, shows that the strain on the horizontal tie is constant throughout.

I find that the simplest forms for the values of F and F_2, are found by eliminating among equations (20) and placing the results in the following form:

$$F_2 \cos\theta = \frac{V - \Sigma_0^x P - H_1 \tan i}{1 - \tan\theta \tan i} \qquad . \qquad . \qquad (28)$$

$$F \sin i = \frac{V - \Sigma_0^x P - H_1 \cot i}{\cot\theta \cot i - 1} \qquad . \qquad . \qquad (29)$$

In the article referred to above it was shown [equations (13*a*) and (14)] that,

$$\tan\theta = \frac{lN^2}{4Dn(N-n)}, \text{ and } \tan i = \frac{4D}{lN^2}(N - 2n + 1) \quad (30)$$

Equations (26), (27) and (30) will reduce (28) to

$$F_2 \cos\theta = 0;$$

hence there is no strain on the diagonal ties for an uniform load throughout. It is evident that the strain on each of the vertical ties is p, and that they transmit the strains from the lower chord to the arch.

Making $F_2 = 0$ in the first of (20) and we have $F \cos i = H_1$ which is a simpler equation, for this particular case, than (29). It corresponds with the first of (16).

This discussion shows that to produce strains on the diagonal bars it is necessary to load the bridge over only a part of its length, or else unequally load the joints.

If the diagonal bars act as ties, and the bridge be loaded for maximum shearing, we have for the strains on the n^{th} tie, [see equation (17), page 229 of the April number],

$$F_2 \cos\theta = \frac{(N - n + 1)(n - 1)}{2N} p \qquad . \qquad . \qquad (31)$$

If the diagonal ties sustain the load which is at their lower ends, we must add p to the above expression.

Let $n - 1 = n^1 \quad \therefore -n + 1 = -n^1$ and (31) becomes

$$F_2 \cos\theta = \frac{(N - n')n'}{2N} p \qquad . \qquad . \qquad . \qquad (31')$$

which is the *form* taken by the equation when the bars act as braces. (See eq. 15, p. 228 in the April No.)

Equation (31') shows that *the strains vary as the product of the segments into which the span is divided by the vertical bar which passes through the upper end of the inclined tie.*

It is easy to show that the strain on the horizontal chord is greatest when the bridge is completely loaded; hence, for an uniform load we have for the greatest stress on this chord

$$\text{H}_1 = \frac{pl\text{N}^2}{8\text{D}}, \text{ as given in equation (27).}$$

Similarly, the greatest strain on the arch is for a full load; and if the load be uniform, we have

$$\text{F} \cos i = \text{H}_1 = \frac{pl\text{N}^2}{8\text{D}} \quad . \quad . \quad : \qquad (32)$$

as given above.

From (30) we may find

$$\cos i = \frac{l\text{N}^2}{\sqrt{l^2\text{N}^4 + 16\text{D}^2(\text{N} - 2n + 1)^2}}, \quad . \qquad (33)$$

which reduces (32) to

$$\text{F} = \frac{p}{8\text{D}}\sqrt{l^2\text{N}^4 + 16\text{D}^2(\text{N} - 2n + 1)^2} \quad . \qquad (34)$$

From (30) we may also find that

$$\frac{1}{\cos \theta} = \frac{\sqrt{16\text{D}^2\text{N}^2(\text{N} - n)^2 + l^2\text{N}^2}}{4\text{D}n(\text{N} - n)}$$

which reduces (31) to

$$\text{F}_2 = \frac{(\text{N} - n + 1)(n - 1)\sqrt{16\text{D}^2\text{N}^2(\text{N} - n)^2 + l^2\text{N}^4}}{8\text{D}\text{N}n(\text{N} - n)}\, p \qquad (35)$$

Equations (27), (34) and (35) are necessary and sufficient for solving the problem.

Before leaving this problem it will be well to observe that for a load uniformly distributed over the whole length $\text{V} = \frac{1}{2}\text{W} = \frac{1}{2}\text{N}p$. But in this case only one-half the load on each of the end panels causes strains on the truss, because said half is sustained directly by the abutments, or supports. Making this deduction and we have for the re-action which causes strains $\text{V} = \frac{1}{2}(\text{N} - 1)p$ as given in equation (26).

In order to bring together, in this connexion, all the equations for computing the strains on this truss, I copy the following from the article above referred to.

For maximum shearing in the panel system when the load is on the *upper* chord, we have

$$\text{F}_2 \cos i = \frac{(\text{N} - n)n}{2\text{N}}\, p \quad . \quad . \qquad (36)$$

For maximum strain on the upper and lower chords use equations (27) and (32).

In the *Triangular System*, for maximum shearing, when the load is on the upper chord, we have

$$F_2 \cos \theta = \frac{(N-n)^2}{2N} \left[\frac{4n^2-1}{4nN-4n^2-1} \right] p, \quad . \quad (37)$$

with the load on the lower chord, we have

$$F_2 \cos \theta = \frac{(N-n)(N-n+1)}{2N} \left[\frac{4n^2-1}{4nN-4n^2-1} \right] p \quad (38)$$

$$\tan \theta = \frac{lN^2}{2D[N^2-(N-2n-1)^2]} \quad . \quad . \quad (39)$$

Equations (27) and (32) may also be used for this system, for finding the strains on the chords.

15°. *Let both chords be horizontal.*

For this make $i=0$ in equations (20) and they become,

$$\left. \begin{aligned} H + F_2 \sin \theta &= H_1 \\ F_2 \cos \theta &= V - \Sigma_0^x P \\ HD &= Vx_1 - \Sigma_0^x Px \end{aligned} \right\} \quad . \quad . \quad . \quad (40)$$

in which D is the total depth.

The second of these equations shows that the ties or braces, as the case may be, resist all the shearing stress.

For an uniform load $V = \frac{1}{2}wL$, and at the middle section $\Sigma_0^x P = \frac{1}{2}wL$, which in (40) gives $F_2 = 0$, which shows that no ties or braces are needed at the middle. But it will be shown hereafter, that if the uniform load consists of equal weights placed on the joints, there will, in some cases, be a strain on the middle braces.

LET US FIRST CONSIDER A SYSTEM OF TRIANGULAR TRUSSING—see Fig. 12.

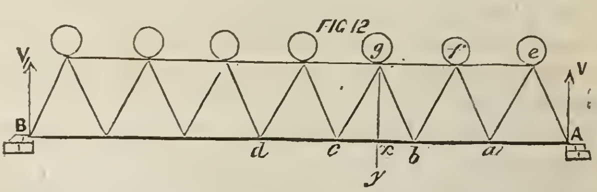


FIG 12

Let the bays be of equal length, and the bars be equally inclined.

Let the equal weights be on the joints of the upper chord. Then the total load is Np. $\therefore V = \frac{1}{2}Np$.

Let a vertical section be made just at the left of g, and call bc the n^{th} bay counting from A; or to be more specific, let n be the number of bays between the end and the *foot* of the brace considered. Then we readily find for the section gx,

$$\Sigma_0^x P = np$$

$$x_1 = (n - \tfrac{1}{2})\, l$$

$$\Sigma_0^x P^x = \tfrac{1}{2} n (n-1)\, pl.$$

These in (40) give,

$$\left.\begin{aligned} H + F_2 \sin\theta &= H_1 \\ F_2 \cos\theta &= \tfrac{1}{2}p\,(N - 2n) \\ H_1 D &= \tfrac{1}{4}\left[N\,(2n - 1) - 2n\,(n - 1)\right]pl \end{aligned}\right\} \qquad (41)$$

We also find $\tan\theta = \frac{l}{2D}$; $\sin\theta = \frac{l}{\sqrt{4D^2+l^2}}$; $\cos = \theta \frac{2D}{\sqrt{4D^2+l^2}}$.

For the braces which incline the same way as $g\,c$, $\sin\theta$ will be negative; for all others it is positive.

If N is even, we have $n = \frac{1}{2}N$ for the middle braces, which in the second of (41) gives $F_2 = 0$; or there is no strain on the middle braces: but if N is odd, we shall have $n = \frac{1}{2}(N + 1)$ or $= \frac{1}{2}(N - 1)$ for the middle braces. These values in the second of (41) will give $F_2 \sin\theta = \pm \frac{1}{2}p$. For the end braces $n = 0$, which gives $F_2 \sin\theta = \frac{1}{2}Np = V$.

For maximum shearing let all the joints on the left of g be loaded and those on the right be unloaded. Then we readily find:

$$\text{the total load} = (N-n)\,p,$$

$$V = \frac{(N-n)^2}{2N}p,$$

$$\Sigma_0^x P = 0,$$

$$\Sigma_0^x P\,x = 0,$$

$$x_1 = (n-\tfrac{1}{2})\,l.$$

These substituted in (40) give,

$$\left.\begin{aligned} H + F_2 \sin\theta &= H_1 \\ F_2 \cos\theta &= \frac{(N-n)^2}{2N}p \\ H\ D &= \frac{(N-n)^2}{4N}(2n-1)l \end{aligned}\right\} \qquad (42)$$

But to combine the cases of uniform load throughout, and an uniform load to produce maximum shearing, we will suppose that the uniform load is the weight of the frame, and let $w_1 = W \div N =$ the weight of a bay in length of the frame. Then if $p = w_1$ in (41) those equations will give the strains, and by the aid of (42) we have for the strains,

$$\left.\begin{aligned} H + F_2 \sin\theta &= H_1 \\ F_2 \cos\theta &= \tfrac{1}{2}w_1(N - 2n) + \frac{(N-n)^2}{2N}p \\ H_1 D &= \tfrac{1}{4}l\left[\left\{N(2n-1) - 2n(n-1)\right\}w_1 + \frac{(N-n)^2}{N}(2n-1)p\right] \end{aligned}\right\} \qquad (43)$$

For an uniform load throughout including the weight of the frame we readily have from (41)

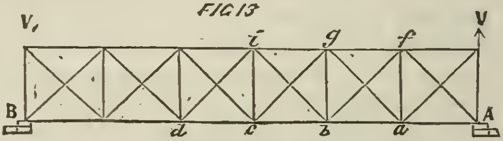
$$\left.\begin{aligned} H + F_2 \sin\theta &= H_1 \\ F_2 \cos\theta &= \tfrac{1}{2}(N - 2n)(p + w_1) \\ H_1 D &= \frac{l}{4}\left[N(2n-1) - 2n(n-1)\right](p + w_1) \end{aligned}\right\} \qquad (44)$$

The third of (44) is greater than the third of (43); hence, for the strains upon the chords we use equations (44). For n less than $\frac{1}{2}$ N, the second of (43) is greater than the second of (44); hence, we use the former to find the strains upon the braces.

Similar expressions might be found when the joints of the lower chord are loaded, but we will omit them and pass immediately to the

PANEL SYSTEM.

FIG 13



For a definition of this system see number 8° of this article. Let figure 13 represent the case. Many bridges have been made in this country upon this principle; among which I will mention the following:

Howe's Truss, in which the chords and inclined bars are made of wood, and the vertical ties are of iron, (See this *Journal* vol. iii. 3d series, p. 289, also, *Silliman's Journal* vol. xviii. p. 123): Pratt's Truss, in which the chords and vertical struts are of wood, and the inclined bars are iron ties, (see Vose's Hand-book of Engineering, p. 154): Long's Truss, which is composed entirely of wood, and the inclined bars are braces, (see this *Journal* vol. v, 2d series, p. 231.): Whipple's Truss, which is composed entirely of iron, and the inclined bars are ties, (see Appleton's Dictionary of Mechanics—the last edition—article Bridges,) and Jone's Truss, which is also made entirely of iron, (see *Scientific American* for 1863, vol. ix, p. 193.) These Trusses differ in the *details* of their construction and not in their mathematical properties, and as it is only the latter that we propose to discuss we shall not dwell upon their points of distinction. Many other trusses partake of some of the properties of those above mentioned.

We now proceed to discuss the theory. The strains upon the diagonal bars will be the same whether they act as ties or braces; also the strain upon them will be the same whether the load be on the upper or lower chord. But the strain on the vertical ties (or struts, as the case may be) is not the same for the load on either chord.

It may be seen from the second of (40) that the *vertical components* of the strains on the several braces which are between two consecutive weights, are equal to each other. For, V being constant, the first member will remain constant as long as Σ_0^x P is constant, and this is constant between two consecutive weights.

From this we see that when the load is on the lower chord, the *vertical components* of A f and $f\,a$ Fig. 13, will equal each other; so of $a\,g$ and $g\,b$. But if the load be on the upper chord, the vertical components of $f\,a$ and $a\,g$ equal each other; so of $g\,b$ and $b\,i$. If ties be used instead of braces, and the load be on the upper chord, then will the vertical components on fb and bg equal each other; but if the load be on the lower chord, the vertical components on af and fb equal each other. If there be no load between e and A, the vertical components will be the same on all the bars between e and A.

Let n be the number of the panel counting from A, in which the strains are considered, which will be the same as the number of a brace (or tie) counting from the same end, and let the other notation be the same as given above.

Then for an uniform load in which the weight of the bridge is considered, we have

$$(\mathrm{N}-1)(p+w_1) = \text{the total load},$$
$$\mathrm{V} = \tfrac{1}{2}(\mathrm{N}-1)(p+w_1),$$
$$\Sigma_0^{x_0}\mathrm{P} = (n-1)(p+w_1),$$
$$x_1 = n\,l \text{ for a section just at the right of } g,$$
$$\Sigma_0^{x}\ \mathrm{P}\,x = \tfrac{1}{2}\,n\,l\,(n-1)(p+w_1).$$

These in (40), give

$$\left.\begin{aligned} \mathrm{H} + \mathrm{F}_2 \sin\theta &= \mathrm{H}_1 \\ \mathrm{F}_2 \cos\theta &= \tfrac{1}{2}(\mathrm{N}-2n+1)(p+w_1) \\ \mathrm{H}_1\mathrm{D} &= \tfrac{1}{2}(\mathrm{N}-n)\,nl\,(p+w_1) \end{aligned}\right\} \quad . \qquad (45).$$

The third of these equations shows *that the strain on the lower chord varies as the product of the segment into which the span is divided by the vertical bar which passes through the upper end of the brace* (*or tie*) *which belongs to the panel considered.*

For maximum shearing there will be $(n-1)$ unloaded joints, and hence $(\mathrm{N}-n)$ loaded joints, but still the weight of the frame must remain as an uniform load throughout. For this case we readily find, in the same way that we found equations (44) that,

$$\left.\begin{aligned} \mathrm{H} + \mathrm{F}_2 \sin\theta &= \mathrm{H}_1 \\ \mathrm{F}_2 \cos\theta &= \tfrac{1}{2}(\mathrm{N}-2n+1)\,w_1 + \frac{(\mathrm{N}-n)(\mathrm{N}-n+1)}{2\mathrm{N}}\,p \\ \mathrm{H}_1\mathrm{D} &= \frac{1}{2\mathrm{N}}\Big[(\mathrm{N}-n)\Big(\mathrm{N}w_1+(\mathrm{N}-n+1)p\Big)\Big]\,nl \end{aligned}\right\} (46)$$

For n less than $\tfrac{1}{2}\mathrm{N}$, the second of (45) is less than the second of (46); hence, we use the latter to compute the strains on the braces. For the strain on the chords use (45).

There is a point at which the shearing stress is zero; and it may be found by placing the second of (46) equal zero and solving for n. The general expression thus found is long and inconvenient, but it may be easily shown that there is one value between $\tfrac{1}{2}\mathrm{N}$ and N—this we will call n_0. The other value exceeds N, and hence is inadmissible.

For example, if $p = w_1$, and $\mathrm{N} = 12$ we find $n_0 = 7{\cdot}9$
$p = 2w_1$, and $\mathrm{N} = 10$ we find $n_0 = 6{\cdot}8$

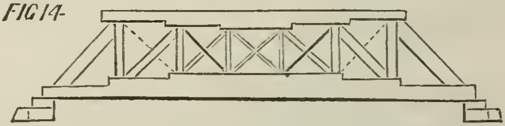
In this way a table might be formed with the arguments p, w_1 and N.

The second of (46) is positive for all values if n between 0 and n_0; and negative for all values between n_0 and N.

To show more clearly the force of this equation, let us suppose that the frame is loaded uniformly throughout, and that it moves off in the direction from A towards B. The strain on Af will be greatest when all the load is on and is found by making $n=1$ in the second of (46); the strain on $a\,g$ is greatest when the rear end of the surcharge has reached a, and is found by making $n=2$ in the same equation: the strain on $b\,i$ is greatest when the rear end has reached b, and is found by making $n=3$; and so on to $n=n_0$, beyond which the expression becomes negative which shows that ties must be used instead of braces; or what is equivalent, the brace must be inclined the other way, like $d\,i$, for instance. The same phenomenon is observed if the load moves from B towards A. We observe that, counting from either end, the braces beyond n_0 incline in an opposite direction to those within that value. From 0 to $\frac{1}{2}$N, or $\frac{1}{2}$ (N + 1), the bars are usually called *main braces* or *main ties*; from $\frac{1}{2}$N to n_0, *counter braces*, because they incline in an opposite way from main braces.

Fig. 14 is a graphical representation of equations (46) and the principles which have here been developed.

FIG 14-



We observe that the first term of the second member, of the second equation of (46) is positive, for n less than $\frac{1}{2}$ (N+1) and negative for n greater than that value, therefore, when the surcharge extends over half or more than half the bridge it conspires with the weight of the frame to produce strains upon the main braces; but they act against each other to produce the strains on the counter-braces. When there is a strain on a counter-brace, there is none on the main brace in the same panel. Between n_0 and N, the effect of the surcharge is merely to relieve the main braces of a portion of the strain which is produced by the weight of the frame alone.

For a queen post truss we have N = 3, in (46);

$$\therefore \quad \mathrm{F}_2 \cos\theta = (2-n)\,w_1 + \tfrac{1}{6}\,(3-n)\,(4-n)\,p,$$

and for the end brace or rafter make $n=1$,

$$\therefore \quad \mathrm{F}_2 \cos\theta = w_1 + p.$$

For the braces in the middle panel, make $n=2$,

$$\therefore \quad \mathrm{F}_2 \cos\theta = \tfrac{1}{3}\,p.$$

Braces are rarely put in the middle panel in the queen post truss, for the stiffness of the frame is relied upon to resist the concentrated load.

For single rafters make N = 2, and $n=1$, in the second of (46);

$$\therefore \quad \mathrm{F}_2 \cos\theta = \tfrac{1}{2}\,(w + p.)$$

16°. The principles just stated are more forcibly illustrated by supposing that the ties are so near each other as to be considered continuous, and that the load is continuous and uniform.

For this, let x = the length of surcharge which is continuous from one end, see Fig. 15,
r = weight of surcharge on a unit of length,
L = length of frame,
w = weight of a unit of length of the frame.

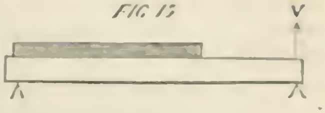


FIG 15

Then
$$V = \tfrac{1}{2}wL + \frac{rx^2}{2L}$$
$$\Sigma_0^x P = w(L - x),$$

and the second of (40) becomes

$$F_2 \cos\theta = \tfrac{1}{2}w(2x - L) + \frac{rx^2}{2L} = y \text{ (say)} \quad . \qquad (47)$$

This may be considered as the equation of a curve.

For
$$x = 0;\ y = -\tfrac{1}{2}wL$$
$$y = 0;\ x = \left(-\frac{w}{r} \pm \sqrt{\frac{w^2}{r^2} + \frac{w}{r}}\right)L \quad . \qquad (48)$$
$$\frac{dy}{dx} = 0;\ x = -\frac{w}{r}.$$

Fig. 16 is a geometrical illustration of equation (47).

From (48) we see that the curve crosses at D, a point between A and the middle of the span. For instance,

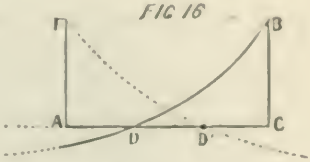


FIG 16

if $r = 10w;\ x = 0{\cdot}231L$
$r = 5w;\ x = 0{\cdot}299L$
$r = 2w;\ x = 0{\cdot}366L$
$r = w;\ x = 0{\cdot}414L$
$r = 0;\ x = \tfrac{1}{2}L$. (See equation (47).

From C to D the braces incline from the support C, and from A to D, they incline from A. If the load moved in the opposite direction, the point of no shearing would be at D′, and between D and D′ braces must incline both ways to resist the strains arising from loads moving both ways.

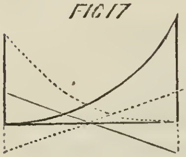
Equation (47) may be separated into two parts; one being the strains due to the permanent load; the other, to the surcharge. Call the former y_1; the latter y_2; and we have

$$y_1 = w(x - \tfrac{1}{2}L.) \quad . \quad . \quad . \quad . \qquad (49)$$

$$y_2 = \frac{rx^2}{2L} \quad . \quad . \quad . \quad . \quad . \qquad (50)$$

Equation (49) shows, that for an uniform load extending over the whole span, the strains increase uniformly from the middle towards the ends.

FIG 17



Equation (50) shows that they increase as the ordinates of a common parabola, counting from one end. The axis of the parabola is perpendicular to the span. Fig. 17, represents equations (49) and (50).

17°. It is desirable to have some easy mode of determining whether a bar is subjected to a thrust or pull. It is evident that the bars of the upper chord are all subjected to a thrust, while those in the lower chord are subjected to a pull or tension. But the inclined bars may be subjected to either, and in varying circumstances to both kinds of strain. To determine to which strain any bar is subjected under any given condition; we observe, first, that the shearing stress is $\left\{\begin{matrix}\text{positive or upwards}\\ \text{negative or downwards}\end{matrix}\right\}$ when the second members of (40), is $\left\{\begin{matrix}\text{positive}\\ \text{negative}\end{matrix}\right\}$. If we conceive the load to be divided by a vertical plane so that the part between A and the plane shall equal V, see fig. 12 or 13, then will the shearing stress between V and the plane, be positive; and beyond the plane it will be negative. Or, this principle may be stated thus: the shearing force will be $\left\{\begin{matrix}\text{positive}\\ \text{negative}\end{matrix}\right\}$ according as the plane section is $\left\{\begin{matrix}\text{nearer}\\ \text{more remote}\end{matrix}\right\}$ than a plane which divides the load into two parts respectively equal to the re-action of the supports.

We next observe that all the bars which are similarly situated on either side of the dividing plan will be subjected to like strains.

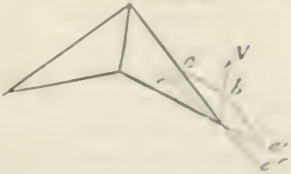
Finally, the brace A*e* (fig. 12, or A*f* fig. 13) is subjected to compression, and its angle of inclination and the shearing stress are both positive; hence *we have this general principle; for ordinary trussed bridges:*

A bar is subjected to a $\left\{\begin{matrix}\textit{thrust}\\ \textit{pull}\end{matrix}\right\}$ *when the sign of its angle of inclination is* $\left\{\begin{matrix}\textit{the same as}\\ \textit{contrary to}\end{matrix}\right\}$ *the sign of the shearing stress on a plane section which passes through the bar.*

This rule which is essentially that given by Rankine for a similar case, (see his Applied Mechanics, p. 161) fails in many cases when applied to trusses of different form. For instance, in the double rafter, as shown in Fig. 18, the inclination of both the right hand rafters is *positive*, and the shearing stress is positive; yet the lower one is subjected to tension.

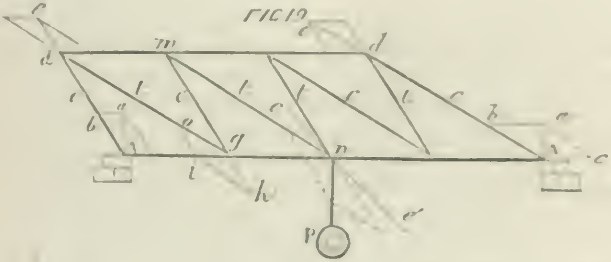
In Fig. 19, all the bars have a *positive* inclination, while the alternate ones are subjected to tension.

FIG 18



Tredgold in his Carpentry (Fourth edition p. 12) gives the following rule to distinguish ties from struts.

FIG 19



"Let a parallelogram be constructed on the direction of the straining force as a diagonal, the sides of the parallelogram being parallel to the sustaining forces; then, let the other diagonal of the parallelogram be drawn; and, parallel to it, draw a line through the point where the directions of the forces meet. Consider towards which side of this line the straining force would move if left at liberty; and all supports on that side will be in a state of compression and all those on the other side will be in a state of tension."

For instance, in Fig. 18, construct a parallelogram on A V as a diagonal, having its sides parallel to A B and A C. Draw *c c'* through A parallel to *a c*. Now if V were free to move it would move vertically upward, and as A B is above *c c'* it would, according to the rule, be compressed and A C extended. The rule draws a correct conclusion in this case; still it is not infallible. For if we suppose there is a single weight acting on the frame, as shown in Fig. 19, and we apply the rule to it, we will find that all the bars at the right which are marked *c*, should be subjected to tension; whereas, they are really subjected to compression. The rule also decides that the lower bar is compressed, whereas it is extended. In justice to the rule, however, I should say that if we suppose that two inclined bars sustained the weight—neglecting the influence of the horizontal tie—it gives correct results. But with this admission I find that it fails in numerous instances in the arched truss. It can then be *relied upon only* in cases where two bars—and only two—are concerned in resisting the force. Tredgold applied it to such cases.

Robison in his Mechanical Philosophy, vol. i. p. 504, gives the following rule: "Take notice of the direction in which the piece acts from which the strain proceeds. Draw a line in that direction *from* the point on which the strain is exerted; and let its length express the magnitude of this action. From its remote extremity draw lines parallel to the pieces on which the strain is exerted. If one of these lines cut the bar, or the bar prolonged in the direction of its remote end, the bar will be compressed; but if it cut the prolongation in the opposite direction it is a tie." This rule is also given in the Encyclopediæ Britanica in the article on Carpentry.

This rule is more nearly general than either of the preceding, and yet it must be used with caution and with some limitations. To illustrate it, refer to Fig. 19, and let Aa represent the re-action of the supports, which is vertically upwards. Through a draw a line parallel to the lower chord; it cuts the bar A d; hence A d is a strut. Now the force in the strut acts from A towards d; therefore, draw $d\,e$ in that direction from d, and proceed as before, and we see that the upper chord is compressed and the next bar extended. Proceed in this way until we come to the point where P is applied: or *generally; commence at either end and follow the rule to the point where the vertical shearing stress is zero.* The limitations to which I wished to refer, is—*we must not pass the point where the vertical shearing stress is zero.*

I have said the rule must be used with caution. For instance, in Fig. 19, to find the strains upon the bars which concentrate at g, we have found that the bar $d\,g$ is a tie, and to produce tension on the part $g\,o$, it must act from g towards o; through o draw $o\,i$ parallel to $m\,g$; it cuts Ag at i; hence, we would be inclined to infer that Ag is compressed, but we found while considering the forces at A, that it was extended. We must therefore infer that the point i belongs to the prolongation of ng; and hence ng is extended. If these cautions and limitations be observed I think that the rule will always give correct results.

My rule has been to observe that the inclined bars commencing at either end, are alternately compressed and extended up to the point where the shearing stress is zero; and that all parts of the upper chord are compressed, and of the lower are extended. It is not difficult to determine whether the first one is extended or compressed, and hence, the rule is easily applied.

In the next article I shall consider some cases in which the axis of Y intersects more than three bars.

(To be Continued.)

Mons. Crepin's Experiments on Baltic Timber Creosoted under Bethell's Process.

[Annales des Travaux Publics de Belgique.]

From the Lond. Practical Mechanic's Journal, August, 1864.

The experiments undertaken by me in 1857, at Ostend, to ascertain the relative preservation of timber prepared with sulphate of copper, and timber prepared with creosote oil, when placed in the sea, and the relative resistance of such differently prepared timber to the attacks of the Teredo worm, have been previously given to the scientific world.

I have proceeded with these experiments, and having again minutely inspected the creosoted wood, I am able to say that it presents no trace of the Teredo, and is in a perfect state of preservation. The experiments, I believe, may be now taken as decisive, and we may conclude that well creosoted fir timber, prepared with creosote oil of good quality, is proof against the attacks of the Teredo, and is certain to last for a long time.

Let us sum up the results of these experiments, or, at least, such as apply to the timber submerged in the sea, and exposed to the Teredo's attacks. This timber is placed exactly 1·90 met. (6 ft. 3 ins.) above the low-water mark of spring tides, so that the pieces between 2 met. and 2·60 met. (6 ft. 7 ins. and 8 ft. 7 ins.) long are left partly dry twice in the day. The Teredo greatly abounds at Ostend, and has been found in all situations below 1·90 met. (6 ft. 3 ins.) above the low-water mark of spring tides.

In the first fortnight of the month of October, 1857, I placed immediately beneath the line marking 1·90 met. (6 ft. 3 ins.) above the water mark, upon the piles of the east pier—

1. Three pieces of creosoted fir, which had been taken from a lot of wood prepared in the ordinary way by the State Railway.
2. Three pieces of beech prepared with sulphate of copper.
3. One piece of fir, and one piece of beech wood, not prepared in any way.

The first Inspection, 1859.—In the beginning of January, 1859, the above mentioned pieces were taken down and inspected.

It was found that the piece of fir unprepared was much perforated by the Teredo; that the piece of beech unprepared was perforated from one end to the other; that the pieces prepared with sulphate of copper were all three eaten by the Teredo; and that the three pieces of creosoted fir alone were intact, and without a trace of the Teredo.

The three last mentioned pieces alone were replaced, a slice having been previously cut off from each as a specimen; in two of the pieces the sawn surface was covered with flat-headed nails, but, in the case of the third, this precaution was purposely omitted.

The second Inspection, 1860.—The second inspection of the three pieces of creosoted wood took place in March, 1860.

Traces of the Teredo were discoverable in the piece, the sawn surface of which had not been covered with the flat-headed nails; but the two other pieces were altogether intact. Upon the heart wood portion of one of the sides, which probably had not been very fully impregnated with creosote, some very small holes of young Teredos were discovered, but it was plain that they had not been able to penetrate it at all. The two latter pieces were replaced.

The third Examination, 1862.—In the early part of July, 1862, the two pieces replaced in 1860 were re-examined, and presented no signs of the Teredo. They were then replaced as before.

The fourth Examination, 1864.—The two pieces replaced in 1862 were examined upon January 21st, 1864, and it was then found that they presented no trace of the Teredo, and no sign of decay of any kind. These two pieces are now in as perfect a state of preservation as when they were first put into the sea, the wood has retained all its elasticity, and has acquired a density which it did not possess in its unprepared state; the creosote oil also appears to have been entering more deeply into the wood. These two pieces have been exposed to the Teredo from the month of October, 1857, to 21st January, 1864, —a period of more than six years.

They had, however, absorbed but a small proportion of creosote oil. They are now in a state of perfect preservation. The experiment appears to me conclusive.

However, as I said in the notice of this experiment inserted in volume xx of the "Annals," the Honorable the Minister of Public Works was so good as to order some pieces of wood to be prepared at Ghent on the 16th May, 1861, by the officers of the State Railway, expressly for my use; and I accordingly received 15 pieces then prepared, with which to make a further experiment. I fixed these 15 pieces in the first fortnight of June, 1861, upon the front row of piles of the east pier, all within 1·90 met. (6 ft. 3 ins.) above the low-water mark of the spring tides.

In the course of the winter of 1861, and the first month of 1862, three of these pieces, numbered 30, 33, and 40, were swept away by the sea, and lost.

The remaining 12 pieces were taken off, placed on the quay, and examined on the 11th July, 1862. After having ascertained that they bore no trace of the Teredo, I kept as samples numbers 16 and 18, and replaced the ten others.

These were taken down and examined on 21st January, 1864, and it was then again ascertained that they showed no trace of the Teredo, nor any kind of alteration or decay.

They are strongly impregnated with creosote, and the oil seems to have been penetrating more deeply into the wood. The wood is hard, and retains all its elasticity. On weighing the 10 pieces in question, I found that they had gained in weight, upon an average, 6·25 kilos. (14 lbs) each, during their immersion from 1862 to 1864. In 1862 we found that they had scarcely increased in weight at all, but the normal weight with which we compared them was, in this case, their weight as taken in the creosoting yard immediately after their preparation; and most probably this normal weight would have been less had they been weighed in the first instance upon their immersion in the sea at Ostend.

At all events, it is certain, that the weight of the creosoted pieces of wood is found to be increased after their immersion in the sea; they cannot therefore have lost in the sea any of the creosote oil with which they were impregnated. This circumstance is probably owing to the insolubility of the oil in the water, and also to the fact of its density being about equal to that of the sea water. It appears that, after its creosoting, and its subsequent immersion in the sea for two years and a half, the fir wood has nearly doubled in weight. It has acquired, and now retains, the density of oak.

This trial of creosoted fir for marine purposes appears to me conclusive, both as regards the preservation of the wood, and as regards its resistence to the Teredo. Experiments made in England, and recently in France and Holland, tend to the same conclusion. I cannot too strongly recommend the use of creosoted fir wood in Hydraulic engineering, in preference to oak (the price of which, especially for the larger pieces, has become excessive), since, in addition to its being cheaper, there is no doubt of the creosoted fir lasting longer. The Government Public Works Department has cordially adopted this most beneficial process, and constructed part of the dyke, and the whole of the American foot passengers' bridge in the new works at Ostend, of creosoted red fir.

At Nieuport, a visitors pier, 600 met. (660 feet), has been built of creosoted fir, upon the left bank of the channel; and the new pier, which is to be carried out from the end of it into the sea, will doubtless likewise be made of creosoted fir. Moreover, various sluice gates at Ostend have recently been ordered to be renewed, and creosoted Baltic fir and pitch pine to be used for that purpose.

The only things about which, to my mind, we need be solicitous, are *the proper creosoting of the timber with proper creosote oil, and the use of the proper kinds of timber*, viz., those best suited to the process of creosoting.

It has been observed that the resinous descriptions of wood become most thoroughly injected, and that the use of white fir should be discountenanced.

I also think it right to mention that, in the case of the sluice gates recently ordered, no limit has been fixed to the quantity of creosote which may be injected into the wood; also, that it is required that the wood be first subjected to a vacuum of 20 per cent. of the barometer for an hour; and immediately after this, have creosote oil forced into it at a pressure of 8 atmospheres during at least two hours.

OSTEND, February 5th, 1864.

MECHANICS, PHYSICS, AND CHEMISTRY.

An Account of Balloon Ascensions. By Mr. JAMES GLAISHER.

From the London Athenæum, Oct., 1864.

The Committee on balloon experiments was appointed last year for the following purposes:—To examine the electrical condition of the air at different heights; to verify the law of the decrease of temperature; and to compare the constants in different states of the atmosphere. With respect to the first of these objects, no progress had been made with the exception of preparing an instrument and apparatus for the investigation. At the request of the Committee, Mr. Fleeming Jenkin undertook the construction of the best instrument for the purpose, and one was finished towards the end of 1863, but it was constructed to be used with fire; it has since had to be adapted for water—a constant flow of which is necessary in electrical experiments in balloons.

This apparatus Mr. Glaisher was requested by the Committee not to use, as they felt that these instruments, if exerting no influence while the balloon was rising, might, when it was falling, throw considerable doubt on the experiments relating to humidity. With respect to the second of these objects, the verifying the law of the decrease of temperature in different states of the atmosphere, the Committee considered would be best attained by taking as many observations as possible, at times in the year, and at times in the day, at which no experiments had been made, for the purpose of determining whether the laws which hold good at noon apply equally well at all other times of the day. The Committee have always pressed the importance of magnetic observations in the higher regions of the air; the Astronomer Royal suggesting the use of a horizontal magnet, and taking the times of its vibration at different elevations, a method which is seldom practicable, owing to the almost constant revolution of the balloon. To obviate this, Dr. Lloyd suggested the use of a dipping-needle, placed horizontally when on the ground, by means of a magnet above it, so that, when in the balloon, the deviation from horizontality might be noticed, and which deviation would be independent of rotary motion of the balloon. The latter method has not yet been tried, Dr. Lloyd wishing some experiments to be made before the instrument was constructed; at Newcastle a very general wish being expressed that very high ascents should not again be attempted, none above five miles had since been made. Mr. Glaisher then gave an account of the ascents made by him during the past year. The first was from Newcastle, on the 31st of August. The balloon left the earth at 6h. 12m. P.M., with a north wind, and descended at five minutes past 7, at Pittington, near Durham. The decrease of temperature within the first 200 feet of the earth in this ascent was very remarkable, no such rapid decrease having been found in any other ascents. On the ground the temperature was 64°, and by the time 200 feet had been attained, a decrease of 8 degrees had taken place, the temperature being 56°; from this height to 1200 feet, there was but little change, and above this the temperature decreased from 2° to 3½° in each succeeding 1000 feet, up to 7000 feet when the balloon entered a relatively warmer current of air. The second ascent, on the 29th of September 1863, was from Wolverhampton. The gas on this occasion had been prepared in July, expressly for a high ascent intended to have taken place before the Newcastle Meeting, but circumstances prevented this being made, and the gas was obligingly stored in the gasometer by the directors of the gas-works. The balloon left at 7h. 43m. A.M., wind S.W. At 8200 feet there were two layers of clouds below the balloon and very dense clouds above. When at 11,000 feet, the clouds were still a mile higher; there was a sea of blue-tinged cloud below, and peeps of the earth was seen through the breaks. At 13,000 feet, high clouds were still above; but after this they began to dissipate, and at 9h. 38m., at 14,000 feet, the sun shone brightly. Ten minutes afterwards the travelers discovered the Wash at a distance of only ten miles, and were compelled to descend. A south-west gale was blowing, and so strong was the wind that on the grapnels taking the ground near Sleaford, at 10h. 30m., the balloon was rent from top to bottom. In this ascent warm currents were met with at 8000 and 13,500 feet. In the descent a warm current was passed through, extending from 14,000 to 9000 feet. Temperature at the ground on leaving 48°; at time of descent 53°. On passing out of the mist at 3000 feet the humidity declined to 58° at 8000 feet; here there were dense clouds above and below; at 9000 feet the humidity was 71°; and then the air became suddenly dry. The third ascent was made from the Crystal Palace, at 4h. 29m., P.M., on the 9th of October. In seventeen minutes it was 7300 feet high, and directly over London Bridge, and all the vast number of buildings comprising the whole of London could be clearly seen. There were neither warm nor cold currents met with on this day. The Secretary of State for War having granted permission to the Committee to avail themselves of the facilities afforded in the Royal Arsenal at Woolwich, the ascent of the 12th of January was made from thence. It was intended to have been made on the 21st of December previous, and from time to time the balloon had been partially inflated. It left at 2h. 7m. P.M., and in 14 minutes had crossed the Tilbury Railway, and was over Hainault Forest; at 3h. 31m. the height of 12,000 feet was attained, when the balloon began to descend and touched the ground at 4h. 10m. at Lakenheath. On the earth the wind was S.E.; at 1300 feet a strong S.W. current was entered, in which the balloon continued up to 4000 feet, when the wind changed to S. At 8000 feet the wind changed to S.S.W., and afterwards to S.S.E. At 11,000 feet, fine granular snow was met with; and the balloon passed through snow on descending, till within 8000 feet of the earth. Clouds were entered at 7000 feet, which merged at about 6000 feet into mist. This ascent is the only one ever made in January for scientific purposes. The fifth ascent was designed to have been made as near the 21st of March as possible, but through adverse weather, was deferred to the 6th of April. The balloon left Woolwich at 4h. 7m. P.M., with a S.E. wind, ascending evenly at the rate of 1000 feet in about three minutes, till 11,000 feet was attained at 4h. 37m. It descended in the Wilderness Park, near Sevenoaks in Kent. Its course was most remarkable, having passed over the Thames into Essex; the balloon, unknown to the aëronauts, must have re-passed the river and moved in a directly opposite direction, and so continued till it approached the earth, when it again moved in the same direction as at first. The ascent is remarkable for the small decrease in temperature with increase of elevation. The air at the period of starting was 45½°, and did not decline at all until after reaching 300 feet, after which it decreased gradually to 33° at 4300 feet. A warm current was then entered, and the temperature increased till 7500 feet was attained, when 40° were attained, being the same as had been experienced at 1500 feet. It then decreased to 34° at 8800 feet, and then increased slowly to 37° at 11,000 feet, a temperature which had been experienced at the heights of 8500, 6500, and 3000 feet in ascending. After the great injury to the balloon on the 29th of September, in addition to the repairs it had previously undergone, Mr. Coxwell did not consider it, after the additional rough usage in the last two voyages, safe for extreme high ascents, and determined to build a new one; which he did, capable of containing 10,000 cubic feet more gas than the old one, so that, if need be, two observers could ascend together to the height of five miles. A new balloon, however, needs trying in low ascents until it proves gas-tight, before it can be used for great elevations; and, on June 13, it was therefore started on a small ascent from the Crystal Palace, at 7 o'clock; the sky cloudless and the air perfectly clear, except in the direction of London. An elevation of 1000 feet was reached in 1¼ minute, 3000 feet at 7h. 8m., when the balloon descended to 2300 feet, and then re-ascended to 3400, when, after a slight dip, it again ascended to 3550 feet, the highest point by 7h. 28m., and then, after some oscillations, began its downward course at 7h. 50m. from 2800 feet, reaching the ground at East Horndon, five miles from Brentwood, at 8h. 14m. The remarkable feature in this voyage being that below 1800 feet elevation there was scarcely any change of temperature until the earth was reached. This fact, of no change in the temperature of the air at the time of sunset, was very remarkable, for it indicated that if such be a law, the law of decrease of temperature with increase of elevation may be reversed at night for some distance from the earth. June 20th the balloon left Derby, at 17 minutes past 6, P.M., and descended near Newark. June 27, the balloon ascended from the Crystal Palace, at 6h. 33½m., the sky cloudy, wind west. The descent was made on Romney Marsh, 5 miles from the shore. These several trial trips of the new balloon were made, and it was gradually becoming gas-tight, when its lamentable destruction at Leicester took place. The mayor of that town has recently presided over a meeting for the purpose of collecting subscriptions to assist Mr. Coxwell to re-build a new balloon; and we concur in Mr. Glaisher's wish that the town of Leicester and the Foresters' Society will soon remove the stigma resting upon them. Mr. Coxwell since then has had recourse to the old balloon, which he had repaired as best he could, and the next and last ascent of which Mr. Glaisher had to speak, was made with it, on August 29, from the Crystal Palace, at 4h. 6m. The difference between the temperatures of the air and those of the dew-point in this ascent was rather remarkable. The most important point in the past year's experiments are:—That though the law of decrease of temperature under ordinary circumstances in the Summer months is pretty well determined, we cannot say such a law holds good throughout the year; nor can we say that the laws which are in force during the day will be in force at night. In carrying out these experiments, Mr. Glaisher said he had freely given up all his leisure, and that Mr. Coxwell had done the same in the most unselfish manner; indeed, had it not been for the generous spirit in which Mr. Coxwell had entered into these experiments, they never could have been made, except at a multiple of the cost that had been incurred.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

From the London Mechanics' Magazine, July, 1864.

[Report of the Chief Engineer, July 26, 1864.]

During the last month 218 engines have been examined, and 327 boilers, 14 of the latter being examined specially, and 6 of them tested with hydraulic pressure. Of the boiler examinations, 259 have been external, 11 internal, and 57 thorough. In the boilers examined, 178 defects have been discovered, 8 of them being dangerous.

No. 17 explosion occurred during last month, but as the investigation into all the circumstances connected with it was not completed when the last monthly report was issued, its consideration was postponed till the present occasion, and may now be entered upon.

Much as has already been stated in these reports as to the treacherous character of externally fired boilers, and the danger attendant upon their use, yet another fatal explosion of one of this class has to be recorded, which occurred, on this occasion, to a boiler under the inspection of this Association. By this explosion three persons were killed, and two others injured, one of them very seriously; while its occurrence only corroborates the views already expressed, and shows the necessity of the warnings which have been given.

The boiler in question was the centre one of a series of three, working at a colliery, all of them being of plain cylindrical egg-ended construction, externally fired, and each set with a flash flue. The length of the boiler was 37 ft., its diameter 6 ft. 6 ins., and the thickness of the plates from 3-8ths to 7-16ths-in.; while the safety valve was loaded to a pressure of 40 lbs. per square inch, which was by no means excessive for a boiler of such dimensions. It rent, as is so usual in these cases, through the line of rivet holes at one of the transverse seams, starting, in the first instance, at the bottom, and then running round the entire shell, and thus separating it into two pieces. In this instance the rupture occurred about mid-way in the length of the boiler, the firing end containing five widths or belts of plate, and being about 17 ft. 6 ins. long, and the chimney end containing six widths of plate, and being about 19 ft. 6 ins. long. These portions of the boiler flew in opposite directions, the firing end to a distance of about 170 yds. from its original seat, and the chimney end about 130 yards, or upwards of 1-6th of a mile apart.

The greater portion of the water with which the exploded boiler was fed was drawn from the workings of the pit, and was of a sedimentary character; while it was pumped into the boiler cold, and, in addition, the firing appears to have been severe. These are trying conditions for an externally fired boiler, and it is not, therefore, surprising that the seams of rivets over the fire gave way repeatedly; indeed such frequent repair was needed that as much as £200, as the proprietor stated at the inquest, had been expended in consequence on two of the boilers, though only three years old, the third one being new, and not as yet set to work. Only a few months ago the exploded boiler had been newly bottomed at the furnace end for about half its length, five of the belts or widths of plate at the part over the fire having been renewed, and it was where the new work joined on to the old that the rent commenced; the rent running from rivet hole to rivet hole through the old plate which formed the inner overlap, while the new work did not give way. This rent did not run round the entire seam of rivets instantaneously, but opened for a short distance at the bottom in the first instance, and remained so for some days previous to the explosion. The attendants became aware of its existence from the leakage that occurred, and, on the Sunday previous to the explosion, the foreman in charge of the boilers got inside the one in question and, assisted by the smith, patched it in the following way:—In the first instance, they drove out four of the rivets, then laid over the crack a plate, which they daubed with horse dung, in order to make good the joint and stop the leakage, fastening the plate down by four bolts passed through the holes in the shell from which they had previously knocked out the rivets. In this plastered state the boiler worked on from the Monday morning till the following Saturday, when the explosion took place.

This does not appear to have been the first, but the third time that this system of patching had been adopted with this boiler, the identical plate used in this instance having been employed before for the same purpose, the foreman stating that he had been shown how to do it by the boiler-makers when previously executing some repairs at the colliery.

To those who still continue the use of these externally fired boilers the recommendations previously given can only be repeated, viz:—

1st. Heat the feed-water before its introduction to the boiler, and disperse it by means of a perforated pipe carried horizontally for several feet near to the surface of the water, and thus prevent its impingement on any particular spot, especially near the firing end.

2d. Where the water is at all sedimentary an efficient blow-out apparatus should be attached and regularly used. Surface blowing out by means of a scum pipe is particularly adapted to externally fired boilers.

3d. Do not allow the flames to act too intensely on any one spot, but spread the action over as extended a surface as possible, and lower the fire bars should any signs of distress appear at the seams of rivets.

4th. Have a spare boiler, so that defects may be suitably repaired immediately on detection, and the boilers regularly cleaned out without the necessity of Sunday work, a practice which is unadvisable though regarded only from an engineering point of view, is demoralizing in its influence upon the workmen, and expensive to the steam user.

5th. With regard to the construction of the boiler, secure good workmanship and material in the first instance. It is a mistake to consider that externally fired boilers are better for being made of thick plates. Those under inspection which have given the least trouble have not been more than three-eighths of an inch in thickness, and it is thought that in plain cylindrical boilers this thickness should not be exceeded. In effecting repairs, as well as in the first construction, the rivet holes should be brought fairly one over the other, without straining, while drifting should not be allowed. In putting in new plates to old boilers, it will in many cases be advisable to cut away the old line of rivet holes and drill new ones; while the new work should rather be thinner than the old, instead of being thicker, as it sometimes is. These are, however, but ameliorative measures; while it is thought a more radical cure would be found in condemning these plain cylindrical externally fired boilers altogether and adopting those internally fired in their place. It surely is no argument to object that the Lancashire type of boiler, though successfully introduced into mills, is inapplicable to colliery purposes on account of the rough usage to which it would be exposed, since this is to make carelessness its own excuse, and to put a stop to all improvement. Although it can scarcely be necessary, yet it may perhaps be stated, in conclusion, that this Association must receive the co-operation of its members if it is to protect their boilers from explosion, and all its responsibility must clearly cease, if between the times of its periodical inspection, the boilers are allowed, as in the present instance, to be patched and plastered without its knowledge.

Lecture on Light. By Professor Roscoe.

From the London Mechanics' Magazine, September, 1864.
Read before the British Association.

The light emitted by burning magnesium wire is exceedingly rich in chemical rays—so much so that this light may be used instead of sunlight for the purpose of photography. As an illustration of this I will endeavor, with Mr. Brother's assistance, to take a photograph of our President, if he will kindly permit me to do so. [Sir Charles Lyell here took a seat in front of the white board, and, by the light of burning magnesium wire, a negative was taken, from which a print was obtained in sufficient time to enable a capital positive to be thrown upon the screen by the aid of the oxyhydrogen light before the audience dispersed. The result was loudly cheered.] The spectrum of burning magnesium is exceedingly rich in violet and ultra-violet rays, partly due to the incandescent vapor of magnesia, and partly to the intensely heated magnesia formed by the combustion. So long ago as 1859 the chemically active power possessed by this light, compared with that of the sun, was determined, and the use of this light for the purpose of photography was proposed. It was shown that a burning surface of magnesium wire, which seen from a point at the sea level, has an apparent magnitude equal to that of the sun, effects on that point the same amount of chemical action as the sun would do if shining from a cloudless sky at a height of 99 degs. 53 mins. above the horizon. On comparing the visible brightness of these two sources of light, it was found that the brightness of the sun's disk as measured by the eye, is 524 times as great as that of burning magnesium when the sun's zenith distance is 67 degs. 22 mins.; whilst at the same zenith distance the sun's chemical brightness is only five times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent. In the memoirs published in the above year it is stated that the steady and equable light evolved by magnesium wire burning in the air, and the immense chemical action thus produced, render this source of light valuable as a simple means of obtaining a given quantity of chemical illumination, and that the combustion of this metal constitutes so definite and simple a source of light for the purposes of photo-chemical measurement, that the wide distribution of magnesium becomes desirable. The application of this metal as a source of light may become even of technical importance. A burning magnesium wire, of the thickness of 0·297 millimetre, evolves as much light as 74 stearine candles, of which 5 go to the pound. If this light lasted one minute, 0·987 metre of wire, weighing 0·120 gramme, would be burnt. In order to produce a light equal to 74 stearine candles burning for 20 hours, whereby about 20 lbs. of stearine is consumed, 72·02 grammes (two-and-a-half ounces) of magnesium would be required. The magnesium wire can be easily prepared by forcing out the metal from a heated steel press having a fine opening at bottom; this wire might be rolled up in coils upon a spindle, which could be made to revolve by clock-work, and thus the end of the wire guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit lamp flame, in which it would burn.

The manufacture of this metal is now carried on under Mr. Sonstadt's patent, by the Magnesium Metal Company of Manchester, and the light evolved by the burning wire is likely to be applied to very many technical purposes.

Some persons are sanguine enough to believe that it may be used for illuminating our streets, and I understand that the gentlemen who manufactured the metal have an application from the managers of the gas works at Rio de Janeiro, who seem to think it would be better for them there to light with magnesium wire instead of gas. I believe this is a serious proposal, but we can scarcely expect that it will be carried out in practice. In conclusion, I have only to thank you for the very kind way in which you have listened to me.

Gun Cotton.

From the London Mechanics' Magazine, September, 1864.

Messrs. Pelouze and Maurey, in a memoir presented at the last meeting of the Paris Academy of Sciences, state it as their opinion, derived from a long and careful investigation of the subject of gun cotton, that that explosive compound, if now better known as far as its composition, mode of production, and properties are concerned, is still, with regard to its employment in fire-arms, in the same position as it was in 1846. "Nothing, in fact authorizes us to believe that it is possible, in the present state of our knowledge, either to prevent its spontaneous combustion or to correct in a practical manner it liability to burst the weapons at present used for gunpowder."

On Machinery for the Manufacture of Plate Glass. By Mr. George H. Daglish, of St. Helen's.

From Newton's London Journal of Arts, July, 1864

Within the last ten years the production of plate glass in England has been quadrupled, whilst in the same time the price has been diminished fully one-half. The present extent of the manufacture in this country is about 85,000 square feet per week, whilst about 12,000 square feet per week of foreign plate glass is imported. The foreign glass has obtained a preference, from its superior lightness of color, which arises from the greater purity of the materials that it is made of, particularly with regard to the sand, of which the foreign makers have an abundant supply, of great purity and light color, as seen from the exhibited specimens of English and foreign sand.

Under the influence of competition, the English manufacturers have lately commenced an extensive course of experiments, with the view of improving the quality of their plate glass, and also reducing the cost of manufacture; and in some instances very decided success has thus far been the result. In order to accomplish these objects, the sand employed at the British Plate Glass Works at Ravenhead, near St. Helen's, is now imported from France; and every precaution is adopted to ensure as far as possible the chemical purity of the other ingredients of the glass. At these works, also, two of Mr. Siemen's regenerative gas furnaces have been erected for melting the materials; and, from the absence of smoke and dust in them, and the facilities they afford for regulating the heat, these furnaces have contributed greatly to the desired results. Under these altered circumstances, the glass now manufactured is fully equal to the best samples of the French production.

As time is money, any improvement which tends to expedite the manufacture of glass is of importance. This is strongly exemplified in the process of annealing. After the materials have undergone the process of melting in the furnace, and are considered in a fit state for casting, the pot containing the melted mass is taken to the casting table, and its contents poured out on one end of the table, in front of a large cast iron roller; the material is then spread out by passing the roller over the surface of the table; the thickness of the plate of glass being regulated by strips of iron placed along each side of the table, on which the ends of the roller run. As soon as the plate of glass is sufficiently solidified to bear removal, it is introduced into an annealing oven, there to be gradually reduced in temperature or "annealed," until it is fit to be exposed to the atmosphere without risk of fracture. This process of annealing used formerly to occupy upwards of a fortnight; but, from the improved arrangement and construction of the annealing oven, it is now completed in four days. Thus three times the quantity of glass can now be annealed in each oven, compared with what was formerly considered possible; and consequently a large outlay in building and in space has been saved, since only one layer of plates can be placed in the oven at one time; no method of piling the plates being considered practicable, or even safe. The chemical difficulties and manipulation in producing the raw material have thus been very satisfactorily overcome; but the problem of carrying out the necessary improvements in the subsequent mechanical operations has not perhaps been so completely solved, though considerable strides have been made in that direction also.

The plates of glass, when taken from the annealing ovens, are exceedingly irregular, particularly on the surface which has been uppermost in the process of casting,—that surface being undulated or wavy after the passage of the roller over it whilst in a semi-fluid state; the lower side, too, is affected by any irregularities on the surface of the casting table, and also to some extent by the floor of the annealing oven; and both sides of the plates are also covered with a hard skin, semi-opaque. The plates vary in size, the largest being about 17 feet long by 9½ feet wide, and the thickness varies, according to the size, from ⅜-inch to ⅝-inch. The first process to which the plates are submitted is that of grinding, to take off the hard skin and reduce the surface to a uniform plane, which is performed by the application of sand and water. The second process is that of smoothing, which is a continuation of the first process, but performed with emery of seven different degrees of fineness, so as to prepare the surface of the glass for the final process of polishing. This last process is effected by the use of oxide of iron employed in a moist state.

The machine in general use for grinding is that which was originally employed at the commencement of the glass manufacture, and is believed to have been designed by James Watt. It is known by the name of the "fly-frame," machine. It consists of two benches of stone, sufficiently large to hold a plate of glass, and placed about 12 feet apart: on these benches the plates of glass are fixed by plaster of Paris. Each bench has a runner frame made of wood, about 8 feet long by 4½ feet wide, shod on the under side with plates of iron about 4 ins. broad and ¼-inch thick, and provided with a strong wrought iron stud on the upper side, by which it is moved about over the surface of the glass. The gearing for driving these two runner frames is placed between the two benches, and consists of a square cast iron fly frame, with two flat bars hinged to it on opposite sides, extending over each bench, and suspended from the roof by long chains, so as to allow them to radiate freely in every direction: this is called the "fly frame," from the peculiar motion given to it, and each of the runner frames is connected to it by a central stud working loosely in the slot between the flat bars. The fly frame receives its motion from an upright spindle, which is driven from the main line of shafting by a pair of bevil wheels, with a friction clutch for throwing in and out of gear. On the top of the spindle is a wrought iron arm or crank carrying a movable stud, which works in a bush in the centre of the fly frame. Round the centre spindle are also four other spindles, equidistant from the centre spindle and from one another, each carrying on the top a wrought iron crank arm, with movable stud, similar to the centre one: these studs severally work in bushes at each corner of the fly frame. Hence, when motion is given to the centre spindle, the fly frame is carried round by the stud on the crank arm, while its sides are always kept parallel to their original position by the four corner cranks. The two runner frames, being connected by their central stud to the arms of the fly frame, receive the same circular motion as the fly frame; but at the same time they are left free to revolve round their own centres, which they do in a greater or less degree, according to the varying friction of the grinding surfaces. The grinding motion being thus obtained, sand and water are constantly applied, until the surface of the glass is found, upon examination, to be free from all defects; the sand is then washed off the glass, and the first stage of the smoothing process is commenced on the same machine, by substituting the coarser qualities of emery in place of the sand. The plate of glass is then removed from the bench, turned over and replaced on the bench, and submitted to the same process on the other side. The speed at which the fly frame is driven is about forty revolutions per minute. As the runner frame is not sufficiently large to act upon the entire surface of a large plate of glass at one time, it is necessary to divide the operation, and shift the position of the runner frame as the work requires it, by inserting the centre stud of the runner frame in a different portion of the slot between the fly frame bars.

Until the last few years, the principal part of the operation of smoothing was effected by manual labor,—the operation being performed by rubbing two pieces of glass together, and applying emery powder between them. Great care is requisite, as the work approaches completion, that no scratching shall take place; and it is on this account that hand labor is considered absolutely necessary for finishing the process, the slightest scratch being immediately felt by a practised hand, whilst a single stray particle of grit on a machine would spoil the whole surface before it was perceived. About 1857 Mr. Crossley introduced a machine for smoothing the plates of glass, which so far succeeded, that the nicety of the hand-touch is only required for the final part of the operation. This smoothing machine is exceedingly simple and inexpensive. It consists of a long wooden bar, connected at one end to a crank on an upright spindle, and extending over the stone bench on which the plate of glass is laid: two runner frames of wood are attached to the bar, and on the under side of each frame is fixed another plate of glass; these are then laid upon the glass on the bench. In this case the runner frames are only allowed to partake of the motion given to them by the bar, and are not left free to revolve round their own centres, as in the grinding operation previously described. The centre of the bar between the two runner frames is kept in position by a radius rod secured to a fixed bracket on one side of the bench, at right angles to the direction of the bar. The crank being set in motion, the bar and runner frames receive a movement somewhat similar to the figure 8, which is very similar to the motion given in manual labor. One advantage of this machine is, that two surfaces of glass are finished at one operation. The space between the two runner frames is found very convenient for applying the emery, and also ascertaining the progress of the work, without having to stop the machine.

The machinery used in the polishing process remains the same in principle as that originally constructed for the purpose. Each machine consists of a strong cast iron frame, about eighteen feet long by ten feet wide, containing a series of small rollers, upon which is placed a wooden table with two racks on the under side; suitable gearing is connected to these racks, to give the table a slow alternate lateral motion, so as to bring every part of the plate of glass under the action of the rubbers or polishers. The plates of glass are fixed upon the table by plaster of Paris, and the ends of the table move between slide blocks secured to the main frame, so as to prevent the action of the rubbers from displacing it. The rubber blocks are pieces of wood, covered with felt, and provided with a central spindle and adjustable weights, to regulate the amount of friction; a number of these blocks are secured to two movable bars, running on rollers, at each end of the table, and driven by a short shaft, with cranks at the ends, set at right angles to each other. The rubber blocks are thus worked transversely to the motion of the table; and by applying the polishing powder in a liquid state, the surface of the glass is gradually brought up to the requisite degree of polish, both sides of the plate successively being subjected to the same operation.

About 1857, experiments were commenced at the British Plate Glass Works at Ravenhead, with an entirely different class of machinery for grinding and smoothing plate glass, with the object of increasing the production, reducing the cost, and also completing the process of smoothing upon the same machine on which the glass is ground, so as to obviate the necessity of a separate machine for smoothing, and also save the expense and loss of time in removing and refixing the plates of glass. The new grinding and smoothing machine consists of a revolving table, twenty feet diameter, fixed upon a strong cast iron spindle, and running at an average speed of twenty-five revolutions per minute, driven through an intermediate upright shaft from the main line of shafting by a pair of bevil wheels, and friction cone, for throwing in and out of gear. This arrangement of gearing for driving the table was made by Mr. Daglish, and was adopted in order to obtain a spindle for the table of a length equal to the semi-diameter of the table, and at the same time to keep the main line of shafting continuous for driving a series of tables in one room. Over the top of the table a strong timber bar is fixed, about ten inches from its surface; and on the two opposite sides of this bar are bolted two notched plates of cast iron, one on each side of the centre of the table. The notches are for receiving the centre studs of the runner frames, which are very similar to those used on the old class of machinery; and the runners can thus readily be moved nearer to or further from the centre of the table, as circumstances require, by shifting the stud into a different notch. The only motion which these runner frames have is round their own centres, and this is given to them by the excess of friction on the side furthest from the centre of the table over that on the side nearest to the centre, this excess being caused by the greater velocity of the portion of the table further from the centre. It is evident that the amount of grinding action is considerably greater on this machine than upon the old one, both from the increased velocity of the runner frames themselves, and also from the double amount of movement obtained by the revolution of the table and the runner frames. The idea of driving the runner frames themselves, as well as the table, was conceived at an early stage of the experiment; but on being put to the test, it was found that the unaided movement of the runner frames adapted itself to the work to be performed far better than any compulsory motion could do. It has also the advantage of leaving the surface of the table free and unencumbered with any machinery, and consequently facilitates the operation of laying and removing the plates of glass: the whole of the driving machinery is also covered over, and thus protected from the injurious effects of the sand and water thrown off from the edge of the table in working.

This machine has been found to answer equally well for smoothing as for grinding; and this is perhaps its most successful feature in a commercial and economical point of view. Both these processes are now completed on it at the Ravenhead Glass Works; the finishing portion of the smoothing operation alone being effected by manual labor for the reasons before stated. The plates of glass being generally oblong in form, it was found that the machine in its original shape, having a circular table for carrying the glass, entailed considerable waste in filling up the area of each table for grinding; and it was then determined to alter the shape to that of an unequal-sided octagon, or square with the corners taken off. No difficulty has been experienced in the process of grinding from this alteration in form, whilst the amount of waste in making up the tables has been considerably reduced, and greater facilities are obtained for grinding large plates. The amount of wear and tear on this machine has been found to be very small in comparison with the old machines, owing to the small number of working parts, the large extent of bearing surface, the smoothness of the motion, and the complete balancing of the table. The quantity of glass finished upon one of these machines per week is from 1200 to 1500 square feet, which is about one-third more than the old machines are capable of doing, due allowance being made for the difference of area in them.

The first point to which attention should be directed for working out further improvements is the method adopted in casting the plates of glass, and the machinery employed to carry out the process. It has been stated that the plates of glass in their rough state are very irregular, so much so that about 40 per cent. of the glass is ground away in the subsequent processes, which is a serious waste of material, and entails a great expenditure of time and material in the process of grinding; it is therefore worthy of consideration, whether some improvement may not be carried out in this direction by obtaining the plates of glass smoother in the first instance. The grinding and smoothing operations are believed to be now improved upon the previous practice, though there is no doubt room for further practical suggestions and appliances. The polishing process has been tried to a limited extent on the revolving table last described, but without any practical advantage: the present system is, no doubt, theoretically correct, as the action of the rubbers is regular and uniform over the whole surface of the glass, thus keeping up a uniform temperature; but some motion producing a continuous movement of the rubbers, instead of the present alternate movement, would no doubt reduce the wear and tear, and require less power, and would probably also be found capable of a higher velocity, resulting in an increase of production, provided the other requisite conditions of the process were complied with.

Mr. Windus exhibited specimens of the plate glass from the several stages of the manufacture, in the rough state after casting, and after each of the processes of grinding, smoothing, and polishing. He also showed specimens of plate glass made from English and from French sand, together with the samples of the sand from which they were made, showing the lighter color and greater purity of the French sand.

Mr. F. J. Bramwell observed that reference had been made in the paper to the highly satisfactory working of Mr. Siemen's regenerative gas furnace, as applied for melting the materials to make the glass at the Ravenhead Works: he had, as engineer to the company, recommended the adoption of that furnace for the purpose, being convinced of the great advantages that would be found to attend its use, and the first furnace on that construction had now been in constant work for fifteen months, and a second and larger furnace had been erected in May last, which had also been in constant work since that time. These furnaces, he believed, left nothing to be desired as far as regarded the melting; but in other respects he thought the process of making plate glass was at present in a most unsatisfactory position, and some improvements seemed to be much wanted in the mechanical contrivances used in the manufacture, though he must admit it was more easy to make that assertion than to show how the improvements were to be effected. A serious objection to the present arrangement was the great amount of handling that the plates of glass had to undergo in the several processes, which was evidently an important point when it was considered that the large plates fetched a higher price per square foot than smaller ones, and therefore it was desirable to avoid the risk of having to cut up large plates into smaller sizes on account of fractures. Under the present methods, however, the risk of fracture was great, from the number of times the plates were handled: on leaving the annealing oven, the plate was handled once in conveying it to the grinding machine and bedding it there, and afterwards a second time in turning it over for grinding the second side; and similarly it had to be twice handled for each of the subsequent processes of smoothing and polishing,—making six times of handling altogether before the plate of glass was finished on the machines; after which it had still to be twice handled in the final operation of hand cleaning. All these processes, he considered, ought to be effected without more than twice laying the plate,—once for each side; or even without laying the plate at all, by working on both sides of it simultaneously: and in this respect, therefore, he thought there was a wide field open for improvements in the plate-glass manufacture.

The revolving grinding table that had been described was a decided improvement upon the old fly frame grinding machine, since, in all mechanical operations, it was better to get rid of a reciprocating action wherever practicable, and replace it by a continuous circular motion. The new construction of grinding table was preferable to the old grinding benches, on account of its protecting all the machinery below it, so that the working parts and bearings were not exposed to injury from the grit thrown off profusely from the grinding table. A further advantage was the large size of the table, twenty feet diameter, which afforded room for working on the whole surface of a large plate of glass at once.

A serious cause of loss at present in the manufacture was the very large proportion of the glass that had to be removed in the process of grinding in order to obtain a level surface of the glass. The undulations on the surface of the plates before grinding could not, he considered, be produced by the roller on the casting table, as had been suggested, because the roller was of great weight and was moved forwards steadily, running at each end on a smooth strip of iron laid along each side of the casting table, by which the thickness of the plate of glass was determined: the surface of the glass appeared level before the plate was put into the annealing oven. The undulations, after annealing, were not in parallel furrows across the plates, but were in the form of hills and hollows, altogether irregular in size and position. It therefore appeared that the glass, in annealing, must contract irregularly, causing this unevenness of the surface, particularly on the side which had laid uppermost in the annealing oven; in consequence of which a large proportion of the glass had to be ground away as waste, in order to obtain a level surface. In the old annealing ovens, the plates had to be left a long time till the oven had cooled down of itself; but the ovens were now built with air channels under the bed, through which a current of cold air passed; so that the heat was reduced as quickly as was practicable without injury to the glass, whereby a great saving of time was effected. No method, however, had yet been devised for laying the plates one on another in the annealing oven, and consequently a large area of surface was required in the ovens, in order to lay them all separately, some of the ovens being as much as fifty feet long, for the purpose of annealing six large plates of glass at a time. The ovens were well designed for uniformity of heat in all parts, notwithstanding their great size.

Mr. R. Pilkington thought the reason of the uneven surface produced on some of the glass plates in annealing was, that the oven was made too hot for the first plates that were put into it, in order that it might be hot enough for the last ones; and consequently the earlier plates had not become sufficiently set with a uniform degree of hardness before going into the oven. The glass certainly went into the oven quite smooth, and the unevenness must therefore arise in the annealing. At the St. Helen's Plate Glass Works they made what was known as Hartley's plate, fluted plates of glass, half-inch thick, and these were annealed by being piled on the edge in a vertical annealing oven, as they were strong enough to stand on the edge in consequence of their corrugated form. A plan had also been tried of laying the flat plates one upon another in the annealing oven, by means of a lowering table in the oven; but the objection to this plan was, that if the plates were at all too hot, they stuck together, and could not be separated again without breaking.

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, No. 233.

(Continued from page 325.)

6. *Thomas Hobbes.*—On the discovery of the Torricellian vacuum, philosophers became divided into two sects, called respectively "Vacuists" and "Plenists," the former maintaining that a vacuum was possible and capable of being obtained by certain physical processes, the latter that the world was everywhere full, and the production of a vacuum impossible. Among the Vacuists were Otto von Guericke, Pascal, Boyle, and the greater number of the experimental philosophers who were in the habit of meeting at Gresham College for the discussion of scientific matters; and among the Plenists were Marsennus, Noel, Thomas Hobbes, Franciscus, Linus, and the Cartesians:—of these latter Boyle writes "the subtilest and wariest champions for a plenum I have yet met with."

We have previously* spoken of Noel and Marsennus, and we have now to consider the opinions of Thomas Hobbes.†

Of the theological and mathematical writings of Hobbes I can have nothing to say here, but I consider that views put forward in his philosophical treatises, in so far as they relate to the air, are far less advanced than those of his contemporaries—less advanced, indeed, than those of many of the ancient philosophers who preceded him by nearly twenty centuries. Although intimately acquainted with Bacon an Galileo, the fathers of experimental philosophy, he was himself no experimental philosopher, but he preferred to prove his assertions by reasoning rather than by well founded observation and experiment. He deprecated the trying of experiments, and was specially bitter against the recently formed Society of Experimental Philosophers, the object of which was to do away with the baseless structure of the old speculative philosophy and to found a new philosophy resting on a solid experimental basis.

The language which Hobbes employs in controversy abounds with vituperation, and is in every way utterly unbecoming a philosopher. His arguments are frequently puerile and without weight, and the objections which he urges in order to prove the fallacy of a theory or experiment are often paltry and trifling. In the following words* he addresses Dr. Ward and Dr. Wallis, who had opposed some of his views:—"But I here dismiss you both together. So go your ways, you uncivil Ecclesiastics, inhumane Divines, Dedoctors of morality, unasinous Colleagues, egregious pair of Issachars, most wretched Vindices† and Indices Academiarum, and remember Vespasian's law,‡ that it is uncivil to give ill language first, but civil and lawful to return it.

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* See the third of these papers, Chem. News, vol. viii., p. 246.

† Born 1588. Died 1679.

Hobbes himself did not always remember the first sentence of the law, but assuredly he never forgot the last; it is right to add, however, that the learned Savilian professors were by no means so tolerant in the controversy as they might have been, and although they did not make use of such contumelious epithets as Hobbes employs, they occasionally made rather offensive puns on his name:—it is curious, in a treatise devoted to the explanation of some problem in the higher mathematics, to find such words as "*hob*-goblin."

Hobbes did not turn his attention to natural philosophy till somewhat late in life; his adoption of the views of the Plenists may probably be accounted for by the fact that he was well acquainted with Mersennus and Des Cartes, two of the most notorious of the Plenists. In some of his physical views he was almost a Cartesian.

Hobbes did not admit the condensation or rarefaction of matter; he believed the whole world to be full, and that it could not be fuller than full or less full than full.

He conceived that the air consists of an ethereal substance extending as far as the sun, in which ether, myriads of hard atoms possessing simple circular motion of their own, move; these atoms exist in greater quantity near the surface of the earth than at some distance above it. He denied that the air possesses either weight or elasticity. "Utraque illa phantasia," he writes, "tum gravitatis atmosphæræ, tum vis elasticæ sive antitupiæ§ aëris, somnium erat."

Hobbes adduces a very simple experiment to prove the universality of a Plenum, an experiment for the production of which there was required neither expensive apparatus nor careful manipulation, for the sole apparatus was a gardener's watering-pot, and the experiment was made whenever it was used. The watering-pot, which was in use 200 years ago, consisted of a circular vessel A, the bottom of which, B, was pierced with a number of minute holes; it was terminated above by an opening, C, capable of being closed by the finger. Water would obviously be retained in such a vessel by the pressure of the air as long as C remained closed, but directly it was opened water would flow from the bottom of the vessel. That no water flows from A until C is opened is, according to Hobbes, positive proof of a plenum, for, he reasons, as the world is full, there is no place for the water to go into until C is unstopped, when the air, which is dispaced by the descending water, enters and takes the place of the water which flows from the vessel. "And this," he writes, "I take for a sign that all space is full; for without this the natural motion of the water, which is a heavy body downwards, would not be hindered."*

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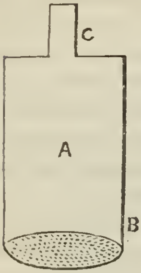
* See "Lessons on the Principles of Geometry, to the Egregious Professors of the Mathematics, one of Geometry, the other of Astronomy, in the Chairs set up by the noble and learned Sir Henry Savile in the University of Oxford." Lesson vii., "On Manners."

† Ward had written a work entitled "Vindiciæ Academiarum."

‡ Hobbes alludes to the following saying attributed to Vespasian, "Maledici senatoribus non oportere; remaledicere fas et civile esse."

§ From αντιτυπτεω, repel a blow, to re-act; a scarcely appropriate term to apply to an elastic body; inasmuch as elasticity signifies that property possessed by the atoms of certain bodies, in virtue of which when their relative positions is altered by the addition of force, or the removal of force previously acting, they return to the position which they occupied prior to the addition or subtraction of that force.



As Hobbes was a plenist, he of course contended that Boyle's air-pump did not produce a vacuum. He speaks of it as being "of the nature of a pop-gun which children use, but great, costly, and more ingenious," and he maintains that the effects produced by it are such as would be produced by a strong wind cooped up in a narrow room. In the first place, when the piston is drawn from one end of the cylinder to the other, he affirms that no air is removed, but sufficient "pure air" to fill the space deserted by the piston enters between it and the surface of the cylinder; for, although the piston was closely fitted into the cylinder with well-oiled leather, it may, he says, keep out "straws and feathers," but not air, "for the body of leather will give passage both to air and water, as you will confess when you ride in rainy and windy weather." (It was by trifling arguments of this kind that Hobbes sought to confute results obtained by one of the most careful and persevering experimenters of the day.) The pure air which forcibly enters between the piston and the surface of the cylinder generates a most violent motion of the air within the air-pump receiver, and this violently-agitated air produces all the effects observed in the receiver after long-continued pumping.

Animals suffer death in the receiver because the rapid motion of the air stops the passage of their blood, but here Hobbes' argument is greatly at fault, for he states elsewhere that the air supports life, because when we breathe we draw in a great number of the swiftly-moving atoms in the air, which pass into the blood, and by their motion cause it to circulate through the veins and arteries; if this be the case, and the above theory obtains, animals ought obviously to have a quicker circulation of blood in the receiver; thus one theory destroys the other, and one is nullified before it leaves the hands of its propounder.

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* See "The English Works of Thomas Hobbes, of Malmesbury, now first collected and edited by Sir William Molesworth." Eleven vols. 1839-1845.

Boyle* has very ably answered all the objections which Hobbes raises against his "Physico-Mechanical Experiments," and the instruments which he employed for their production, and although first attacked, and somewhat violently too, he carries on the controversy in a far more tolerant spirit than that exhibited by his adversary.

As in the air-pump Hobbes admitted no rarefaction of the air, so in the air-gun† he admitted no condensation. By working the piston, he contends, large numbers of the hard atoms in the air are forced into the gun, while the pure air escapes between the surfaces of the piston and the syringe; the motion of the injected hard particles becomes greater and greater with every stroke of the piston, and finally when the valve is opened the particles rush out violently, and carry with them a bullet or anything else in their path.

Hobbes, in common with the Plenists, did not allow that the space above the mercury in the Torricellian experiment is a vacuum; his explanation of the suspension of the mercury in the tube is very difficult to understand; he seems to have thought that the weight of the mercury column presses a certain amount of air into the upper part of the tube, and that the column comes to rest when the weight of the mercury is "equal to the force which is required in air to go through it."

It had long been known that nitre when thrown on red hot coals engenders very rapid combustion; Hobbes explains this by supposing that nitre consists of "many orbs of salt filled with air," which air rushing violently from the nitre meets particles issuing from the burning coal in an opposite direction, and the two contrary motions produce the intense inflammation observed.

Hobbes was well aware that in many coal mines there exists a kind of matter which suffocates living creatures, and extinguishes flame; he speaks of it as "a certain matter of a middle nature between water and air," and as being perfectly transparent and not much lighter than water; elsewhere he speaks of the deadly fumes given off by burning charcoal, as consisting of a "flameless glowing fire, which dissipates those atoms that maintain the circulation of the blood." I do not find, however, that he traces any connexion between the suffocating gas of coal mines and the suffocating gas of burning charcoal.

Hobbes believed winds to be produced by clouds which descend by their own weight, and strike down the air beneath them; when the dejected masses of air reach the earth they rebound, and thus lateral winds moving in all directions are generated. Thunder and lightning are caused by air enclosed in hollow frozen clouds, which air being more and more pressed, at length the ethereal part of it passes out of the cloud, but a great number of hard particles endued with violent motion remain, and ultimately exert such force that the ice is suddenly broken, and the noise produced by the disruption is thunder.

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* See "An Examen of Mr. T. Hobbes, his Dialogus Physicus de Natura Aeris. As far as it concerns Mr. R. Boyle's book of new experiments touching the spring of the air, &c., By the Author of those Experiments." London. 1662. Also, "Animadversions upon Mr. Hobbes' Problemata de Vacuo." By the Hon. Robert Boyle, Fellow of the Royal Society. London. 1674.

† The air-gun was invented by M. Marin Bourgeois in 1607. The first account of it is given in a work entitled "Les Elemens de l'Artillerie: concernans tant la theorie que la pratique du Canon. Augmentes en cette nouvelle edition et enrichis de l'invention, description, et demonstration d'une nouvelle artillerie que ne se charge que d'air ou d'eau pure, et a neanmoins une incroiable force, &c. Le tout par le sieur de Flurance Rivault. Paris, 1608." There is a good copy of this curious work in the British Museum Library.

The learned editor of Hobbes' works speaks of him as "one of the greatest and most original thinkers in the English language." We speak here solely of his merits as a physical philosopher, and I think it will be admitted that he was neither a great physicist nor a man calculated to forward the interests of the then young experimental philosophy. But was it to be expected that the old speculative philosophy would be overthrown at the first onset? was it to be expected that its disciples would see their fabric destroyed without one blow in its defence? a radical change is never made without opposition, never established without a stuggle, and the more violently that struggle the more surely and completely will the change be effected. Hobbes was a follower of the old philosophy, now about to be expelled from the human mind: he lived at the period of the change: should he not rise in the defence of that for which he had devoted his best energies? should he not make an effort to stem that tide which was so rapidly gaining ground?

Men like Hobbes (and we find many such in the history of Science), who rather than allow the falsity of an old and favorite theory, frame a more false theory to support it, even such men indirectly benefit Experimental Philosophy, for by propounding a fallacious theory, they cause experiments to be made for its refutation; and no experiment is made in vain, each one is another stone added to the great and rapidly-increasing structure of our Philosophy.

(To be Continued.)

Specification of a Patent granted to Max Pattenkofer, *for restoring the Surface of Pictures in oil without any danger to their Original state.*—[Dated 20th October, 1863.

From Newton's London Journal of Arts, July, 1864.

Observation and experiment show that the changed appearance noticed in varnished pictures in oil, after the lapse of some years, is caused in most cases by physical and not chemical influences. Time causes in these objects the discontinuance of molecular cohesion. The process begins on the surface with microscopical fissures in the varnish, and penetrates by and by through the different coats of colors to the very foundation. The surface and body of such a picture becomes in the course of time intimately mixed with air, and reflects light like powdered glass, or loses its transparency like oil intimately mixed with water or air. The best method of rejoining the separated molecules without any danger or damage to the original, is the following:—The picture is exposed to an atmosphere saturated with vapor of alcohol at the ordinary temperature (without applying heat). The resinous particles of the picture absorb alcohol from that atmosphere until they are saturated, and not more. By this process the different separated molecules re-acquire cohesion with each other, and the optical effect of the orginal is restored soley by self-action, the picture not getting touched at all. The very small quantity of alcohol absorbed evaporates very soon when exposed to the ordinary atmosphere, and the surface of the picture remains clear as long as a newly-varnished surface.

The most serviceable and convenient apparatus for the above purpose is some wooden case of the size required, about three inches deep, lined inside with some metal, for instance, zinc, with the exception of the lid, to which the picture or the pictures to be restored are fastened by screws in the usual manner, as in ordinary packing cases. Alcohol is then poured in the metal-lined bottom of the box or case, and the lid is shut, thus suspending the pictures face downward over the alcohol. From time to time the lid is opened, in order to watch the progress of the restoration, and to take out those paintings that have absorbed sufficient vapor. For treating a painting that could not well be removed from its place, a case without lid and metal lining is required a little larger than the painting, the bottom of which is covered inside with some absorbing stuff—for instance, flannel or the like—which is slightly sprinkled over, and thus only just moistened with alcohol, and then the case is fastened over the painting, so as to cover it completely. Of course a variety of appliances might be contrived, but they need not be described, since the newly discovered principle of self-action by vapors only is the subject of the invention. Thus, in the same manner, other substances can be used instead of alcohol—for instance, wood-naphtha, ether, sulphuric and other ethers, turpentine, petroleum, and benzine, and the various ottos; and in special cases higher and lower temperature is to be employed, but all that is but secondary and subordinate to the principle of self-action by absorption of vapor.

On Magneto-Electricity, and its Application to Lighthouse Purposes.

By F. H. Holmes.

From the Journal of the Society of Arts, Dec., 1863

As this is a paper on the Application of Magneto-Electricity to Lighthouses, I will begin by saying a few words on lighthouses themselves, their former and present state, and the systems now generally followed in the arrangements for lighting.

Formerly lighthouses were few, and were very nearly all coal fires on high cliffs or towers, and most of them were the property of private individuals; but, as shipping increased, so the lighthouse system became more and more developed, both in the number of lighthouses and in the improvement of those already existing. The coal fire gradually gave way to the oil lamp and candles; next we find the introduction of spherical mirrors or reflectors, and these, again, were superseded by parabolic reflectors, sometimes to the number of more than thirty in one lantern. After this came the introduction of the "Fresnel Lens," which took the place of the reflectors and their lamps, however numerous they might be, and required instead one central lamp.

This "Fresnel Lens" has again grown, so to speak, larger and larger, as the want of a more powerful light was felt, till it has now a diameter of six feet and a height of ten, for to increase the quantity of light the size of the lamp must be increased, and the lens in proportion, or it would have been so far out of focus that the intention of the lens would have been frustrated.

To make these progressive improvements in lighthouses vast sums of money had to be expended; and now let us see what was the end sought. First to improve the light itself. This is done by the substitution of a lamp of four concentric wicks, the largest nearly four inches in diameter, for the coal fire. If the improvement had stopped at that it would have been small indeed, but this lamp is more under command than the coal fire. The value of the introduction of oil is not so much, then, on account of its greater power as for its aptitude for the employment of economizing apparatus, whether this consists of reflectors or lenses. All incandescent bodies give out rays as it were from the centre to the circumference of a sphere; of such rays only those which fall on the sea would be useful to the mariner, but by means of reflectors those rays which would pass inland, or upwards, or downwards, are reflected towards any required point, and by a proper arrangement of a series of reflectors, the whole or nearly the whole of the rays directed where required. The Fresnel lens consists of a middle refracting belt, and a double series of reflecting prisms, or zones, as they are generally termed, and, when properly constructed, it has the property of collecting all the rays into one horizontal beam, so that all the light from the lamp is utilized. Thus, then, we see great strides have been made, since the introduction of oil lamps, as regards the lenticular apparatus—in fact that may be said to be nearly perfect; let us then return to the consideration of the light itself for a moment.

Whether a large or a small lamp be employed it will make no difference in misty weather, so long as the thickness of the flame is the same, for a large lamp may be equal to ten or twelve smaller ones, and, if replaced by these ten smaller, it will be evident that when one of these is obscured by mist the whole of them will be obscured. Quantity of light, then, will not add to its power of penetrating mist. By making the large lamp with four concentric wicks, the intensity of the light is a little increased, and such a lamp will penetrate further through mist in a slight degree. But it is in misty and hazy weather that the light is most required; hence, now that everything else is nearly perfect in a lighthouse, the authorities, both in this country and elsewhere, are directing their attention to the only thing wanting to make the whole system perfect, that is, a light capable of penetrating mist; and as this power depends on the intensity of the light, and electricity is capable of producing the most intense light known, it was naturally looked to as the possible means of perfecting the whole system. But the light produced by electricity to be applicable for lighthouses must be certain and constant, not liable to extinctions or any great variations, as the first would tend to endanger vessels seeking and not finding the light; and if a fixed light had much variation, it might be mistaken for a revolving light.

Let us now see whether electricity can produce a constant, steady, or uniform light. Frictional electricity will give a succession of flashes intensely vivid, and might be used for the purpose, but for the fact that the slightest moisture is sufficient to convey the whole charge to the earth. The various forms of galvanic battery are all capable of producing a steady and intense light, but still (besides the great expense) they are not applicable, because of the necessarily varying current which becomes weaker and weaker as the solution becomes saturated. The magneto-electric machine is then the scource from which one would naturally expect a light which should be invariable in its nature and capable of being continuous for any given time, as the current produced by this machine is constant as long as the helices revolve with the same speed, and the speed can be easily regulated to any required velocity.

The electricity derived from a magneto-machine is induced in coils of wire, by the changing of the magnetic polarity of pieces of soft iron inclosed within the coils or helices; and the quantity or intensity of the induced current depends first, on the amount of magnetism induced in the soft iron; secondly, on the facility with which the poles of the magnetized soft iron can be reversed; thirdly, on the velocity with which the change of polarity takes place; fourthly, on the length and diameter of the wire forming the helices.

The amount of magnetism induced in the soft iron depends on the size and force of the steel magnets employed, and on the weight and softness of the iron in the helices; but the weight in practice of the soft iron is limited by the weight of the steel magnets, for, if too heavy the steel magnets will be slowly deprived of their magnetism. To facilitate the change of the poles the soft iron cores of the helices are not solid pieces of iron, but are tubes, single, double, or treble, as it is found by experiment that the same weight of iron, when divided in this manner, loses or takes magnetism in much less time than when in a solid form.

There is a limit to the velocity to be employed when the maximum of electricity is required, for this reason. It has been already remarked that the amount of electricity depends an the amount of magnetism taken up, and that the soft iron takes time to become saturated, as it may be termed, with magnetism; hence, if the velocity be too great with which the cores move from one pole of a magnet to another, there will not be sufficient time for the cores to become saturated. But as again the quantity of electricity increases as the velocity increases, it is necessary to ascertain this maximum point exactly, which is easily done, either by experiment or calculation, based on certain data. The length and diameter of the wire require to be different, according to the current required; for a short thick wire forming the helices represents a galvanic battery composed of a dozen, say, of very large pairs of plates, whilst a long thin wire would represent a battery composed of thousands of small plates. In other words, supposing the size of the helices to remain the same, if they are composed of thick short wires, quantity is obtained; but if composed of long thin wires, intensity will be the result.

From all this it results that there are certain laws known and established, by which a magneto-electric machine can be made to give a current of any given amount of electricity, with any given ratio between its quantity and intensity.

Having seen on what the production of the current depends, the next point to observe is, the peculiar nature of this induced current. It differs essentially from a galvanic current in this, that while the helices are revolving, the direction of the current is reversed, as the core of soft iron passes each consecutive pole of the steel magnets.

It now remains to explain how the current generated in the wires of the helices is to be withdrawn from the machine. In the first place all the helices are connected in two, or four, or more series, and in doing this great care must be observed that the direction of the coil of every alternate helix is in an opposite direction, that is, if one is wound as a right-hand screw, the next should be as a left-hand screw, or, what amounts to the same thing, supposing all wound in the same direction, then the two inner ends of the wires must be joined of, say, numbers one and two, and the two outer ends of the wires of numbers two and three, and so on through the series; and lastly, the terminals of the series might be soldered into two insulated disks, and then led from the machine by two pieces of metal kept in contact with the outer surfaces of these disks by a slight spring; such an arrangement allows the alternating current to pass from the machine, and such a current will produce a light, but this light has certain disadvantages. It is never white, but always more or less blue or brownish; in fact it is like the electric light obscured by placing it behind a flame from spirits of wine. It is also extremely injurious to the eyes, both from its color and from its tremulousness; I therefore do not use this current but in its stead I convert this constantly inverting current into two that flow from the machine in one direction only. This is accomplished thus: One-half of the helices are arranged so as to arrive on the poles of the magnet at the instant that the other half are exactly midway between the poles. Thus there are two distinct currents; and what may be called the dead point, that is the point when the current inverts in one series, occurs exactly at the time when the other current is at its maximum, so that if now the inverted currents can be again inverted in both of these distinct currents, and that the two now flowing in one direction can be united as one compound current, it is evident that the result will be a current nearly as uniform as that from a galvanic battery, with the advantage of equable continuity.

This is done by the two commutators, which consist each of two insulated rings of metal, of such a form at the periphery that two rollers or rubbers change sides from one disk to the other at the same instant that the current is reversed. Then, by combining the two commutators, a compound current is obtained that will produce a constant white light or perform any of the other functions of the galvanic current, and in a more perfect manner, as it is more uniform in its action.

A steady and constant current thus obtained from the magneto-electric machine is only one part of the problem of producing a constant and steady light, and, although the most important part, still it would be perfectly useless without an efficient lamp or regulator. In order to understand this it is necessary to explain that the carbon points used for producing the light or for converting a portion of the electric current into light, are consumed, and that the rate of consumption is irregular, owing to the irregularities in the structure of the substance used, which is the kind of graphite deposited in the gas retorts sawed up into pencils about a quarter of an inch square; but, as the consumption is irregular, no clock-work with continuous motion could be employed for the purpose of causing the carbons to approach as consumed, for it must be understood that the steadiness of the light as well as its brilliancy depend on the two carbon points being maintained constantly at a certain distance corresponding to the strength of the electric current.

Many pieces of apparatus more or less complicated have been invented from time to time for the purpose of regulating the movements of the carbon electrodes, and many of them I have tried, but none of them, as formerly constructed, could be used in a lighthouse, because they were more or less uncertain in their action, and because the clock-work was too delicate and liable to accident in other hands than those of an electrician. The question, what constitutes a good regulator, must be answered by stating what it must accomplish; and moreover it must perform its several functions in the most simple manner. It must in the first place maintain the carbons at a given distance, whatever be the variation in the state of consumption, and must also be capable of being adjusted to any strength of current; secondly if by any accident the current should be interrupted, and the light thereby extinguished, the regulator should be capable of re-lighting at once with full brilliancy, that is, not only must it allow the carbon points to touch to re-establish the current, but must separate them again instantly, or there would be no light. Such a regulator we have here, for its construction is simple, and it forms its different functions in a most perfect manner. Its construction is this. The upper carbon is attached by a kind of small vice to a bracket, standing out from a tube, which slides freely in a column. The lower carbon is fixed in the end of another tube, exactly under the other carbon. Both of these tubes are put in motion thus: Two cords, passing over pulleys, properly arranged, are wound on one spindle, but in opposite directions. On turning a stud fixed on the end of the spindle the regulator is wound up; that is, the top bracket is raised and the lower tube depressed.

On removing the hand from the stud, the upper tube would descend, and, being loaded, would cause the lower tube to rise; but to prevent this, while the regulator is out of use a bolt is pushed in, which prevents any movement in the regulator till it is again withdrawn. The regulator being wound up, the carbons are firmly fixed in their places by tightening the holders, and are then adjusted so as to bring the points of the focal plane by turning a spindle to which the fixed end of the cord belonging to the lower carbon is attached. So far the regulator is only a means by which the carbons can mutually approach each other with a certain relative speed, depending on the different diameters of the two parts of the spindle around which the cords are wound. But if the carbon points remain in contact, there will be no light. Some contrivance, then, was necessary to separate the points to the distance, which, by experience, is found to give most light, and to maintain that distance between the points constantly till the whole of the pair of carbons is consumed. These two operations are accomplished thus: The fixed end of the cord which works the upper carbon is attached to one end of a lever; the other end of the lever has a piece of soft iron attached to it, over an electric magnet, so that when the bolt is withdrawn, and the carbon runs together until they touch (thus allowing the current to pass), this electro-magnet instantly, by the action of the same current, lifts the cord and with it the upper carbon, to the required distance. But this is not all, for the carbons would again run together were there not some contrivance to prevent them. To accomplish this, advantage is taken of these two facts—first, that the quantity of electricity is proportional inversely to the distance between the carbon points; secondly, that the strength of an electro-magnet is proportional to the quantity of electricity passing through the wire that surrounds it. Bearing these two facts in mind, it will be easy to understand the use of the second electro-magnet. Over this electro-magnet, at a small distance above it, is placed a lever, one end of which is drawn down by a spring, the strength of which can be regulated by a thumbscrew. The fulcrum is between this end and the centre. The other end of the lever is furnished with a catch, and immediately over the electro-magnet a piece of soft iron is fixed in the lever. On the carbons being allowed to touch as before, not only are they separated by the means described, but this second lever, acted on by its electro-magnet at the same instant, is drawn down towards it, and thus brings the catch between the teeth of a wheel placed under it for the purpose, and effectually locks the regulator. The strength of the spring is now adjusted till its tendency to lift the catch out exactly balances the current which draws it down. Should the distance now increase but the $\frac{1}{200}$ of an inch, the spring will be stronger than the current, will lift the catch, and the carbons will approach; by doing so, more current passes, the electro-magnet is strengthened, and is again enabled to overcome the spring and draw down the catch, and thus by their mutual action the distance between the carbon points is all but invariable.

When these regulators are employed in a lighthouse there are a pair for each lens and two small lenses, so that although it may take ten minutes to replace the consumed carbons, still the light is never extinguished; for, suppose the carbons consumed in the lens No. 1, the regulator is ready in lens No. 2; and all the light-keeper has to do is to bolt the No. 1 regulator and draw the bolt of the regulator in No. 2 lens; the current is thus diverted, No. 2 is instantly lighted, and the lighting of this extinguishes No. 1.

Thus, then, we have a most intense light, which may be maintained for any length of time, which does not require to be trimmed or extinguished for a second, and which has all the steadiness and uniformity required for lighthouse purposes. Its advantages over the oil lamp are:—first, its power can be increased *ad libitum* without increasing the size of the lens, for, if required, a machine may be made to give light enough to read by say at 10 or 20 miles; in fact the light is in direct proportion to the power of the machine that produces it; secondly, its great intensity gives it a power of penetrating haze only equaled by the sun; thirdly, its whiteness distinguishes it most perfectly from all other lights on shore, which is one of its most important properties, for many a vessel has been lost for want of this property in lighthouses lighted with oil; fourthly, where colored lights are required for the purpose of distinguishing one lighthouse from another, this light gives all the colors in a perfect manner, while the oil lamp always gives its own tinge to the color employed; fifthly, from the facility with which this light can be extinguished in an instant, and as instantly lighted to its full power, it offers other means of distinguishing lighthouse from lighthouse which cannot be obtained with any other light. The importance of this may be understood from the fact that there are still many points around our shores that require lighthouses, but which must remain without them, 'till better means of distinguishing them with certainty from others in the immediate neighborhood can be employed; for having no lighthouse is hardly worse for the navigator than having two in sight which cannot be distinguished one from the other.

An objection has been made to this light, that, being so small, it would be altogether invisible at a considerable distance; and when we merely consider that the apparent size of distant objects depends on the visual angle, there seems to be some ground for the objection, but the law of visual angles does not apply in the case of self-luminous bodies, as can be demonstrated with this piece of fine wire, which I suppose is almost invisible even with a strong light thrown on it, but now, if by passing a current of electricity through it it is made self-luminous, it appears gradually to increase in diameter as it becomes brighter; and as a curious fact, illustrating the difference between the theorist in his study and the practical observer, a sailor who had seen the magneto-light from a great distance told me he supposed it must be at least ten feet in diameter. Another objection to the light is, that it is too bright; this may be an inconvenience in clear weather, but a light to be useful when most needed must be inconveniently bright in clear weather.

The last point to be considered is the cost of the magneto-electric light as compared with oil. The French director-general of lighthouses has made a report to his government, both as to first cost and as to cost of maintenance; both are greatly in favor of the magneto-electric light; of course in making their calculations of cost, they take the cost of an equal quantity of light in each case, that is by oil and electricity.

I have now only to remark that this invention, if it may be called one, is purely English; Faraday commenced it when he discovered the fact that magnetism might be made to produce or induce an electric current; and although the magneto-light was first produced in Paris, it was by me; and so far from receiving assistance from any of the French *savans* in the matter, I was ridiculed by all of them for attempting what they said they could demonstrate was impossible. With regard to the regulator, which is also invented by me, there is another just invented by a Mons. Foucault, on a very different principle, but which is quite as effective though overloaded with clock-work. His regulator has this peculiarity, it can be used in a rolling vessel, and will bear with impunity the vibrations of a steamer.

Discussion.—Mr. Lawrence suggested that Mr. Holmes should give a description of the practical arrangements of his apparatus at Dungeness lighthouse.

Mr. Holmes stated that at Dungeness, where the light had been in constant use since the 6th of June, 1862, there were in the lantern two small lenses, fixed one over the other, and two regulators to each. Only one light was shown at a time, but there were two regulators for each lens, so that an instantaneous change from one to the other could be made without extinguishing the light when fresh carbons were required. In the machine-room there were two magneto-machines, each capable of giving a powerful light, though both were in constant use. There was a distinct direct-acting steam engine attached to each machine, and there were two Cornish boilers, each capable of generating steam enough for the two engines. The material consumed at Dungeness was about 30 to 35 lbs of coke per hour, and 5½ inches of graphite in the regulator per hour, the price of this last being under three farthings per inch. The principle item of expense was, at present, the engineer, who had charge of the whole apparatus, but he expected that when there were several lighthouses on this principle, it would be found that one engineer would be sufficient for as many as were at present under the charge of an agent, and that none but stokers and light-keepers would be required on the spot. The magneto-electric machines which were at Dungeness contained 120 horse-shoe magnets of about 50 lbs each, and 160 helices, but those which he now constructed contained only from 66 to 70 magnets, and from 88 to 120 helices.

The Chairman said he had listened with much pleasure to Mr. Holmes's very clear statement. He was glad to see present Dr. Gladstone, a member of the lighthouse Commission, and he hoped that gentleman would favor the meeting with some remarks tending to illustrate this subject, which was one of national importance. It was most interesting to know that the little electric spark, not bigger than a pin's head, obtained by Faraday from the magnet not very many years ago, should have led to this development of power in the hands of an able and ingenious man like Mr. Holmes.

Mr. Summerlin referred to an invention of a somewhat similar character, by M. Berlioz, which, he believed, was superior to Mr. Holmes's apparatus. He would have been glad if Mr. Holmes had given some description of it.

Dr. Gladstone, F.R.S., said, as his name had been mentioned by the Chairman, he could not but rise to bear testimony to the able manner in which Mr. Holmes had brought forward this subject. During the existence of the Royal Commission, he had an opportunity of frequently witnessing the experiments made with this apparatus at the South Foreland. The Commissioners afterwards examined everything connected with the lighthouse system in France, where, at that time, when this brilliant light had been burning for half a year at the South Foreland, they were still making preliminary experiments, for they had not then overcome the irregularity of the current of electricity, and could not get a steady light. Since then they had advanced very rapidly, and orders had been given to place a double light of this description at Cape La Hève, near Rouen. The Dutch might perhaps be considered to have been before the French in the adoption of this system, and the Emperor of Brazil was probably before either. He did not think Mr. Holmes had exaggerated the power of this light or the ease with which it was managed, and he was glad to be able to say this, because they knew that an inventor, justly proud of his child, was often unconsciously disposed to give the best possible account of it. He was very glad to hear what Mr. Holmes had said with reference to the expense of an engineer being divided amongst several lighthouses, because that was the main difficulty. When the complexity of an instrument was increased, more skill was required in its management, and this necessarily led to expense. The great desideratum was to have a light which was capable of penetrating to great distances in misty or rainy weather, and in that respect this light was far superior to the Fresnel lamp, which was quite competent to send a light to the extreme horizon on a clear night. In a dense fog no light whatever was of any use; but a mere mist, or a shower of rain, the electric light could penetrate. There was abundant testimony that the lights at Dungeness and the South Foreland had been seen by the captains of steamers crossing from Folkestone to Boulogne, at a far greater distance than an ordinary oil lamp. Moreover, the intensity of this light could be augmented to any extent. Professor Faraday, in his reports to the Trinity House, had laid great stress upon this. All that was necessary was to double the number of magnets, and practically this was easily done, because there were duplicates of everything in such lighthouses, and in foggy weather it was possible to bring the power of both machines to bear upon one instrument; and in that way double the intensity of light could be obtained. Then further,—supposing the fog to be so dense that no light could penetrate it, the steam engine on the premises might be employed to blow a horn or whistle, or to make some other noise which would serve as a direction to vessels. This had been pointed out by M. Regnault, director of lighthouses to the French Government, and was of great importance at a time when the question of fog signals was attracting so much attention.

Dr. Bachhoffner said, having been engaged in most of the patents taken out for producing the electric light, he had some little knowledge of the difficulties which had hitherto been considered almost insurmountable in producing the results which had been shown this evening. The great merit of this plan was the particular mode in which the electric light was obtained, and in this respect, as far as he had seen, Mr. Holmes had displayed a great amount of ingenuity. He confessed, when he first heard that Mr. Holmes had taken this matter in hand, he was very sceptical of his success; he did not believe that so much electric force could be obtained by the magneto-electric machine. The ingenuity displayed was very great, particularly in estimating the exact quantity of iron necessary for the core. Some years ago he (Dr. Bachhoffner) was engaged in some experiments on this subject, but he used iron wire instead of a hollow core. Mr. Holmes had spoken of the existence of liquid carbon between the two points, but this he (Dr. Bachhoffner) would be glad to have more evidence of. In using the electric light there was a deposit on one of the carbons, and with coke points pure graphite was produced, but he had great doubts as to the fact of liquid passing between the two points. A perfect automatic machine for regulating the position of the points was essential, and Mr. Holmes had mentioned that a spring formed part of this apparatus. He (Dr. Bachhoffner) was sorry there was any spring at all, or even clock-work. He thought a lamp might be constructed without a spring, and that would, in his opinion, make the machine perfect. The ingenuity displayed by Mr. Holmes up to this point would, no doubt, enable him to make an improvement in this respect. With regard to the question of cost, he did not think either the oxy-hydrogen light or the electric light would ever be useful for purposes of general illumination. for in such cases the cost of the light was a most important consideration, but for lighthouse purposes this ought not to be regarded, because the matter involved the safety of human life. Nevertheless, it would be interesting to know what was the cost of this system, light for light, as compared with other methods. He congratulated the public, the seafaring portion of it in particular, upon this valuable application of electric power, which at one time he thought hardly possible of accomplishment.

Mr. Holmes said the cost of this light compared with oil had been gone into by M. Regnault, director-general of lighthouses in France, and he had calculated very fairly on the principle of light for light, and, reckoning in this way, including the expenses of alteration, taking down the large lens and putting in two smaller ones, putting up the apparatus, two steam engines complete, and the buildings to contain them, the whole of the cost was calculated at half that of an ordinary first-class lighthouse, light for light. They would quite understand the actual expense was greater than in an ordinary lighthouse, but when the quantity of light was considered, it was less by one-half, whilst the working expenses was only one-third. The light at Dungeness, he calculated, was equal in quantity to 14 of the large oil lamps with four concentric wicks. With regard to the intensity of the light, there was no form of combustion, and no chemical action which could produce a light—explosion was a different thing—beyond a certain amount of intensity. The greatest was that obtained upon lime, because the hydrogen and oxygen gases, burnt together, approached as nearly as possible to an explosion. With regard to the small size of this light, it might be argued that so small a light would become invisible at a great distance, say 30 or 40 miles. If an object three feet in diameter appeared to be only an inch at the distance of a mile, what must this little point of light be at a long distance? He admitted there was something in this argument when based on the theory of the visual angle only, but it did not apply to luminous bodies. [Mr. Holmes illustrated this by showing a thin wire, which was almost invisible till rendered incandescent by a current of electricity, when its apparent diameter was greatly increased.]

The chairman said the next duty which devolved upon him was the agreeable one of proposing a vote of thanks to Mr. Holmes for his valuable paper. He was quite sure they had heard with satisfaction what had been stated by Dr. Gladstone, who had had opportunities of examining on a large scale the methods of illumination at present practised, and he was sure every remark from that gentleman would have great weight with those present. The power which this light possessed of penetrating to a great distance constituted its superiority to any system of lighting now in use; and it was to be borne in mind that that was dependent upon the extreme intensity of the heat evolved. It had been correctly stated by Mr. Holmes that of all the lights produced by chemical means, that of the combustion of hydrogen and oxygen gases upon a ball of lime was the most intense; but electricity was far more intense than any chemical action. By its means they could fuse the most refractory metals, and convert into vapor substances which could not be volatilized by other means. With regard to the precise condition of the carbon as it passed from point to point there might be a difference of opinion. He thought it doubtful whether it was liquefied. He could corroborate the statement of Dr. Bachhoffner as to the complete conversion of the carbon into graphite when coke was employed. Mr. Holmes was greatly to be congratulated on the manner in which he had contrived to economize the power of his currents. He had by an ingenious method detected the means of indicating the exact quantity of magnetism residual in the magnet. Another curious result which Mr. Holmes's practical experience enabled him to effect, was the proportioning the weight of the armature to the size of the steel magnet, so as to avoid diminishing its power. This was one of the most curious results in the science of magnetism that had been produced in the course of this inquiry. An opportunity had been presented to Mr. Holmes of making experiments on a grand scale, which could not be done in the laboratory of the chemist. The practical man followed the theorist, and hence there arose a harmonious co-operation between the two in the advancement of science. He congratulated the Society upon having had so valuable a paper brought before them.

The vote of thanks was then passed.

The paper was illustrated by a display of Mr. Holmes's arrangement of the electric light as used in lighthouses. Some reflectors and oil lamps were kindly lent by Mr. R. C. Wilkins, with the view of showing the various systems employed in lighthouses at different periods.

Pro. Society of Arts, Dec. 2, 1863.

Allen v. *The Steam Boiler Assurance Company.*

From the London Mechanics' Magazine, July, 1864.

This was a somewhat curious action arising out of a recent boiler explosion at Nottingham, in which

Mr. Overend, Q C., and Mr. Wills appeared for the plaintiff; and Mr. Field, Q.C., and Mr. Beasley for the defendant.

For many years past the question as to the causes of boiler explosions has occupied the attention of manufacturers and scientific men, and every effort has been made to render them as unfrequent as possible. They are still, however, so frequent that the defendants have established themselves for the purpose of insuring manufacturers against losses arising from explosions. In their policies the defendants exempt themselves from liability in respect of accidents arising from "wear and tear;" and in the present case the only question was whether the accidents in question arose from "wear and tear," so as to exempt the defendants from liability. The boiler in question was supplied by and put up under the personal superintendence of the defendants engineer. After lasting, however, only three years, instead of ten years, which was said to be the average duration of boiler-life, it burst—happily, however, without causing loss of life, or doing any material damage.

The case on behalf of the plaintiff was that the boiler was faulty in construction and badly erected. It was shown in evidence that the boiler had always leaked from the time it was first put up, and that the iron consequently corroded until it became so weak that a pressure of 13 lbs. to the inch was sufficient to burst it. It was also shown that the boiler was of a kind peculiarly liable to accidents. The base of the boiler was placed in the fire, and the pressure of steam forcing the water upwards, and leaving a steam space at the bottom, the base of the boiler was left without water, and was always, therefore, more liable to corrode and burst than in the case of a boiler the base of which is placed under the fire. It was contended that if the defendants were not liable in the present instance it would be impossible to imagine a case in which they were liable.

For the defendants, evidence was given of scientific men, who gave it as their opinion that the plaintiffs workmen had been negligent in their care of the boiler, and that the accident had arisen from "wear and tear." It was contended for the defendants, that if they were liable in such a case as the present, the result would be that they would have to keep in repair the boilers of all persons who insured with them.

The jury found a verdict for the plaintiff.

On Punching and Drilling Rivet holes.

From the London Mechanics' Magazine, August, 1864.

Probably the liveliest discussion which arose upon the several papers read at the meeting of the Mechanical Engineers at Glasgow, turned upon the comparative advantages of punching and drilling in making rivet holes. Mr. Fletcher, the veteran tool-maker (and a partner in the house of Messrs. William Collier & Co.), had prepared a very able paper, in the course of which he properly gave the preference to drilled as compared with punched rivet holes. He described the defects of ordinary punching, not perhaps with especial originality, inasmuch as these defects are so well known, and have been so often discussed, that little that is new could be said of them. The process of punching is a rough, and we may say, a barbarous mode of making a hole in an iron plate. A series of holes, too, in a row, can hardly be punched with that degree of accuracy which is necessary in modern boiler-making and bridge-building. We are here speaking only of accuracy of line and pitch, for no punched hole is in itself cylindrical or smooth. Mr. Roberts's jacquard punching machine secured, it must be owned, a degree of accuracy before unknown in punching bridge plates, and it was employed with success for punching the plates of the Conway, the Victoria, the Boyne, and the Jumna bridges, and, more recently, upon the Menangle and Penrith bridges for the Great Southern Railway of New South Wales. But admirable as is the ingenuity displayed in the jacquard machine (and that now at the Canada Works, Birkinhead, is the only one which the late Mr. Roberts ever made), it must be owned that it would have accomplished even better work had it been a drilling instead of a punching machine. Besides, whatever want of truth and accuracy there may be in ordinary punching, it is believed that the action of the punch strains, and thus weakens the iron. Few, we think, employ punches which exactly fit their dies, and therefore the operation of punching does not give the clean shearing cut which is commonly counted upon. An examination of a plate which has been only half punched through will at once show how much the punch distorts, and therefore of necessity strains the iron upon which it acts. No iron can be once strained beyond its limit of elasticity without injury, or, in other words, without being made weaker ever afterwards. Yet an ordinary punch does strain the iron surrounding the punched hole, and to an extent which Mr. Fairbairn has estimated pretty closely from the results of experiments upon punched plates. Although with 11-16 in. rivet holes of

1¾ in. pitch, less than 40 per cent. of the iron is removed by punching, the single riveted joint loses at least 44 per cent. of the strength of the solid plate. This, too, is the result, notwithstanding the friction at the lap of the plates, which thus gives each plate a certain hold upon the other. We believe Mr. Fairbairn's experiments showed a loss of strength in the iron left between the holes punched in a row equal to about fifteen per cent. of the strength of the same net section of iron before punching. With very thick plates the injury by punching is greater, we believe than with thin plates. The French engineers prefer very thick iron for large locomotive boilers, and for those, 5 feet in diameter, of the largest Engerth engines, iron of 15 millimetres, or 6-10 in. thickness, is employed. Here we believe the loss of strength by punching amounts to something considerable, and we can say that, in some of the French bridge-work, we have seen three consecutive rivet holes cracked out to the edge of the plate under the combined action of punching and riveting. A series of experiments, made by a committee of Lloyd's, with reference to the strength of the riveted plating of iron ships, went to show that a joint in 3-8 in. iron was absolutely as strong as one in ½-in. iron, the loss of the whole strength, due to riveting, being only 40 per cent. in the first case, and nearly 60 per cent. in the second. If, instead of trusting to general opinions, engineers chose to investigate by actual experiment the effect of punching upon the strength of iron plates, they would, we believe, have reason to abandon punching to a great extent, if not altogether. The drill makes a clean cut, without the chance of injury to the iron, and in this way its employment must add greatly to the strength of riveted structures, and that extra strength is quite irrespective of the additional strength obtained by the certainty that the rivets exactly fit the rivet holes, and that the holes are exactly coincident in opposite plates without the use of the drifting tool. Rivet holes are now drilled with nearly the rapidity with which they can be punched. Mr. Fletcher, at the Glasgow meeting, described a machine upon the general construction adopted a few years ago by Messrs. Cochrane for drilling the plates of the Charing Cross-bridge, and he mentioned the fact that this machine drilled all the holes in a large plate of 1 in. thickness in fourteen or fifteen minutes only, all being drilled at one operation. By powerful hydraulic pressure, worked from an accumulator, each drill is pressed with a force of about 6 cwt. but by increasing the pressure the whole plate may be drilled in the short space of 3½ minutes. Mr. Adamson, the eminent boiler-maker of Hyde, stated that he found the drilled plates much better, in respect of strength and tightness of the joints than those punched in the ordinary manner, and he anticipated that, within a year or so from the present time, every boiler plate worked up in his factory would be drilled instead of being punched. With a far-seeing judgment of the requirements of an improved practice, Mr. Adamson added that he had still greater confidence in welded seams; and we may conclude that this grand improvement, occasionally resorted to by Bury, Hackworth, and others, will eventually come into general use in the case of boilers made of plate iron.

There were those, however, at the Glasgow meeting, who contended stoutly that drilled rivet holes were in no respect superior to those punched in the ordinary manner. We are unwilling to refer personally to the speakers who supported this view, but they had no facts whatever in support of their assertions upon this point, beyond the negative proof that boilers made with punched holes had not yet blown up, nor bridges, with holes thus punched, yet broken down. This argument, however it may commend itself to a certain class of practical men, is not that upon which the question is to be discussed. No one pretends that bridges are in danger of breaking down merely because their rivet holes are punched, and therefore no question of this kind is at issue. The only question is, do punched holes leave as much strength, and are they as consistent with tightness and general excellence of workmanship, as those drilled with the improved machinery? It is no proof because a bridge has not broken that it is as it might be made with the same materials. The necessity for the utmost strength, both of the materials and of their combination in bridges, boilers, and ships, is now more widely recognised and insisted upon than heretofore. Not only are we building numerous bridges of great span, but the weight of railway trains is increasing, not merely when taken collectively, but when weighed over each foot of line covered. So with boilers; while the whole sizes are still retained, there is a tendency to increased pressures; and in the case of iron ships, no amount of strength consistent with lightness is too great. We shall have an additional guarantee of safety when all riveted work, to which the security of human life is confided, is carefully drilled instead of being punched, and yet we have to confess to a wonder that the representatives of great houses should, both at Glasgow and at the late meeting of the South Wales Engineers, contend that punched plates were as strong as, if not stronger than, those drilled by special machinery.—*Engineer.*

Process for Preserving Flowers.

From the London Artizan, Dec., 1864.

Mr. C. R. Tichborne states that, being desirous of preserving a vegetable *lusus naturæ* for some time, he submerged it in some weak glycerine, considering that that fluid would be less likely to destroy the tender organism, and also remembering that it had been found most efficient in the preservation of animal tissues. The glycerine answered its purpose most admirably, preserving the delicate parts of the plant and preventing decomposition. He immediately saw that this property of glycerine might be made available for certain pharmaceutical purposes, where it was desired to preserve or extract the aromata of vegetable products, such as elder, orange, or rose flowers, and also might be substituted for the oils and fats used in the purest process termed enfleurage. The glycerine need not be especially pure, but should be devoid of odor. The elder-flowers should be gathered when the corolla is fully expanded, but not too far gone; they should then be plucked from the stem, and packed firmly in wide mouthed bottles or jars, without crushing them; and the whole should then be covered with glycerine. Mr. Tichborne states that he has thus preserved flowers for two years, and, on distilling them, procured a water the perfume of which has equalled the most recent product. For the preservation of the aroma of the flowers he considers the employment of glycerine far superior to the system termed enfleurage, in which heat is used.

Translated for the Journal of the Franklin Institute.

Welding Iron by Hydraulic Pressure.

M. B. Duportail, an engineer, has been trying experiments upon the application of hydraulic pressure to forging and boiler-making. His experiments were especially directed towards the welding of iron under the hydraulic press, by which a permanent and almost indefinite pressure is produced, by which the welding is effected throughout the mass of the iron, and not merely at a small depth as under the forge hammer. In his last experiments Mr. D. welded, without previous preparation, two pieces of iron, 1½ inches square, after heating them to a white heat. The operation took place with the greatest ease, the iron was moulded like dough, and spread out at the sides whilst the pressure was in action. The pressure was stopped when the thickness at the joint was equal to that of the bars. After cooling, the piece was split in two for the purpose of examining the weld. The appearance was excellent, and one of the two pieces was put under a hammer of a ton and three-quarters and struck three times, cold. The weld showed itself only under the second blow and was not completely opened by the third. *Cosmos.*

Translated for the Journal of the Franklin Institute.

Preparation of Aluminium.

M. Corbelli has found a simpler and more economical process for procuring aluminium than that heretofore used. The metal is prepared from clay first carefully purified from foreign matter,—then dried and treated by an acid to remove iron. About six times its weight of sulphuric acid will answer this purpose. The clay is then allowed to settle, dried again, and mixed with about twice its weight of Prussiate of potassa, the quantity of which is to be increased or diminished according to the content of silica in the clay. To this mixture, one and a half times the weight of the clay, is added of common salt; the mixture placed in a crucible and heated to a white heat. After cooling, the aluminium will be found at the bottom of the crucible. *Cosmos.*

Translated for the Journal of the Franklin Institute.

A New Method of Hardening Cast Iron.

A patent has just been taken out (in France), for a method by which cast iron may be made as hard as tempered steel. When the object in cast iron has been filed up and completely finished, it is to be heated to cherry-red, and plunged until it is cold again in a solution containing 1080 grammes (70 ozs. troy) of sulphuric acid, and 65 grammes (4 ozs. troy) of nitric acid to $2\frac{1}{2}$ gallons of water. The thickness of surface hardened is sufficient for ordinary wear, and the form of the object is not at all altered. *Cosmos.*

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, November 17th, 1864.

William Sellers, President, in the chair.

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

The Board of Managers and Standing Committees reported their minutes.

The Board of Managers reported resolutions passed at their last meeting for regulating the scientific proceedings of the Institute, which were read and approved by the Institute.

In accordance with the second resolution, the President appointed a standing Committee to have charge of the scientific proceedings consisting of the following gentlemen, viz.

Prof. Fairman Rogers, J. Vaughan Merrick, Esq., Prof. John F. Frazer, Coleman Sellers, Esq., and Prof. Henry Morton.

The Special Committees on Weights, Measures, and Coinage, of the United States; on Steam Expansion; and on a uniform system of Screw-threads and Bolt-heads, and Nuts, reported progress.

Robert Briggs, Esq. called the attention of the meeting to the proposed establishment of a new Institution in Boston, Mass., under the title of the "Institution of Technology." He stated the amount of donations already contributed in money and land for the purpose, and submitted two pamphlets published on the subject, one entitled "Objects and Plan of an Institute of Technology including a Society of Arts, a Museum of Arts, and a School of Industrial Science;" and the other, "Scope and Plan of the School of Industrial Science of the Massachusetts Institute of Technology.

Prof. Fairman Rogers made some remarks on the manner of conducting the examinations in the school of Technology at Cambridge, Massachusetts.

Mr. Thomas Shaw called the attention of the Institute to the importance of a uniform system of Sound Signals to be used on railroads, and moved a resolution referring the subject to the Committee on Science and the Arts to be investigated by them, and report to the Institute.

Mr. Samuel Dunseith exhibited specimens of Leather, tanned by his patented process, which is as follows:—

The skins are first treated in the usual manner adopted by practical tanners.

The tanning composition is prepared by making a decoction of the plant known as Golden Rod (Solidago), and dissolving in the same a sufficient quantity of "Cuch," or "Catechu," alum, nitre, and salt.

The skins are then introduced into the tanning liquor, which should be maintained at about 15 degrees barkometer standard, until the tanning is completed. Calf skins and kips can be tanned in from 8 to 14 days, and ox skins in from 20 to 90 days.

Holt's Patent Ribbon Hand Stamp was also exhibited. In this stamp a chemically prepared silk ribbon furnishes the coloring matter, and as it will never dry, gum-up or evaporate, the stamp is always ready for use, and the ordinary troublesome pads, brushes, &c. are dispended with.

J. Harrar's Improved Skate Fastening was exhibited. This fastening is intended as a substitute for the plates heretofore used for securing the shoe to the heel of the boot, the proper adjustment of which is a matter requiring considerable labor.

The improvement consists of a small malleable iron hollow cylinder screw, to adjust which it is only necessary to bore a hole in the boot-heel with a half-inch bit, and screw the cylinder into the same.

Mr. Longstreth exhibited J. M. Naglee's Improved Syphon. The short stem of the syphon is bent at its upper end, and connected to the top of a reservoir, from the lower side of which projects the long stem the latter having a cock at its lower end. At the top of the reservoir is a funnel between which and the reservoir is a cock.

The reservoir and longer stem of the syphon are filled through the funnel, with the fluid to be discharged, and when thus filled contains such a quantity of the fluid that before all has been discharged a partial vacuum is formed in the short stem through which the fluid is thus caused to flow into the reservoir.

ERRATA.

In Vol. xlvii.—June, 1864.—"*Robinson on Arch Truss Girders.*"

Page 363, line 11 from bottom for "*ridged*" read "*rigid.*"
364, " 8 " " " "*toward diminishing*" read "*to diminish.*"
" " 5 " " " "*have their*" read "*has its.*"
365, " 15 " top " "*their*" read "*its.*"
366, " 25 " " " "G.F." read "J.G."
368, " 8 " " " "*diminish*" read "*diminishes.*"

A Comparison of some of the Meteorological Phenomena of NOVEMBER, 1864, *with those of* NOVEMBER, 1863, *and of the same month for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	November, 1864.	November, 1863.	November, for 14 years.
Thermometer—Highest—degree, .	69·00°	69·00°	80·00°
" " date, .	30th.	5th.	1st 1860.
" Warmest day—Mean,	65·17	60·50	72·30
" " " date,	9th.	5th.	9th 1857.
" Lowest—degree, .	25·00	25·00	16·00
" " date, .	24th.	30th.	25th 1860.
" Coldest day—Mean,	28·83	30·50	23·50
" " " date, .	23d.	30th.	25th 1860.
" Mean daily oscillation,	10·80	14·37	13 27
" " " range, .	5·84	6·35	5 71
" Means at 7 A.M . .	41·30	42·45	41·18
" " 2 P.M., .	49.22	52·63	50·29
" " 9 P.M., .	44·57	46·12	44·41
" " for the month,	45·03	47·07	45·29
Barometer—Highest—Inches, .	30·347 in.	30·249 in.	30·661 in.
" " date, . .	25th.	23d.	12th 1851.
" Greatest mean daily press.	30·321	30·232	30·520
" " date, . .	24th.	23d.	12th 1851.
" Lowest—Inches, .	29·080	29·434	29·080
" " date, . .	4th.	17th.	4th 1864.
" Least mean daily press.,	29·150	29·498	29·150
" " date, . .	4th.	17th.	4th 1964.
" Mean daily range, .	0·208	0·185	0·186
" Means at 7 A.M., .	29·848	29·863	29·909
" " 2 P.M., .	29·803	29·804	29·866
" " 9 P.M., .	29·849	29·854	29·897
" " for the month, .	29·833	29·840	29·891
Force of Vapor—Greatest—Inches,	0·548 in.	0·463 in.	0·832 in.
" " " date, .	10th.	15th.	8th 1857.
" " Least—Inches, .	·082	·074	·055
" " " date, .	24th.	10th.	25th 1857.
" " Means at 7 A.M., .	·218	.208	·225
" " " 2 P.M., .	·227	·214	·230
" " " 9 P.M., .	·227	·216	·233
" " " for the month,	·224	·213	·229
Relative Humidity—Greatest—per ct.,	94·0 per c.	97·0 per c.	100·0 per c.
" " " date,	7th.	15th.	often.
" " Least—per ct.,	30·0	25·0	25·0
" " " date, .	30th.	7th.	7th 1863.
" " Means at 7 A.M.,	76·2	73·7	77·4
" " " 2 P.M.,	60·9	52·2	59·0
" " " 9 P.M.,	72·2	66·0	73·5
" " "for the month	69·8	64·0	70·0
Clouds—Number of clear days,* .	5	6	8·2
" " cloudy days, .	25	24	21·8
" Means of sky cov'd at 7 A.M.,	72·7 per c.	57·3 per c.	60·8 per c.
" " " " 2 P.M.,	73·0	54·3	60·9
" " " " 9 P.M.,	59·7	52·3	53·0
" " " for the month.	68·5	54·6	58·2
Rain.—Amount,	4·065 in.	2 960 in.	3·778 in.
No. of days on which Rain fell,	13.	7·	10·6
Prevailing Winds—Times in 1000	N83° 40′W·269	N82°46′W·371	N72°44′W·265

* Sky one-third or less covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the AUTUMN *of* 1864, *with that of* 1863, *and of the same Season for* FOURTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	Autumn, 1864.	Autumn, 1863.	Autumn, for 14 years.
Thermometer—Highest—degree, .	81·00°	83·00°	95·00°
“ “ date, .	Sep. 24th.	Sep. 7th.	Sep. 12th '51
“ Warmest day—Mean,	75·00	77·17	85·20
“ “ “ date,	Sep. 24th.	Sep. 7th.	Sep. 6th 1854
“ Lowest—degree, .	25·00	25·00	16·00
“ “ date, . .	Nov. 24th	Nov. 30th.	Nov. 25th '60
“ Coldest day—Mean,	28·83	30·50	23·30
“ “ “ date, .	Nov. 23d.	Nov. 30th.	Nov. 25th '60
“ Mean daily oscillation,	12·09	15·04	15·10
“ “ “ range, .	5·07	5·17	5·27
“ Means at 7 A. M., .	51·54	51·27	51·66
“ “ 2 P. M., .	59·79	61·73	62·57
“ “ 9 P. M., .	54·69	55·00	55·43
“ “ for the Autumn,	55·34	56·00	56·55
Barometer—Highest—Inches, .	30·347 in.	30·312 in.	30·661 in.
“ “ date, . .	Nov. 25th.	Sep. 23d.	Nov. 12th '51
“ Greatest mean daily press.	30·321	30·244	30·520
“ “ date, . .	Nov. 24th.	Sep. 23d.	Nov. 12th '51
“ Lowest—Inches, . .	29·080	29·281	29·012
“ “ date. . .	Nov. 4th.	Sep. 18th.	Oct. 26th '57
“ Least mean daily press.,	29·150	29·460	29·059
“ “ date, . .	Nov. 4th.	Sep. 18th.	Oct. 26th '57
“ Mean daily range, .	0·147	0·136	0·150
“ Means at 7 A. M., .	29·783	29·908	29·927
“ “ 2 P. M., .	29·746	29·862	29·886
“ “ 9 P. M., .	29·790	29·906	29·912
“ “ for the Autumn,	29·773	29·892	29·908
Force of Vapor—Greatest—Inches,	0·741 in.	0·784 in.	0·991 in.
“ “ “ date, .	Sep. 24th.	Sep. 17th.	Sep. 6th 1854
“ “ Least—Inches, .	·082	·074	055
“ “ “ date, .	Nov. 24th.	Nov. 10th.	Nov. 25th '57
“ “ Means at 7 A. M.,	·313	·311	·336
“ “ “ 2 P. M.,	·317	·324	·354
“ “ “ 9 P. M.,	·327	·328	·355
“ “ “ for the Autumn,	·319	·321	·348
Relative Humidity—Greatest—per ct.,	97·0 per ct.	97·0 per ct.	100·0 per ct.
“ “ “ date,	Sep. 5th.	Nov. 15th.	often.
“ “ Least—per ct.,	30·0	25·0	23·0
“ “ “ date, .	Nov. 30th.	Nov. 7th.	Oct. 21st. '59
“ “ Means at 7 A.M.,	74·5	75·1	77·9
“ “ “ 2 P.M.,	57·5	53·8	57·1
“ “ “ 9 P.M.,	70·8	69·2	73·6
“ “ “ for Autumn,	67·6	66·0	69·5
Clouds—Number of clear days,* .	25	26	29·1
“ “ cloudy days, .	66	65	61·9
“ Means of sky cov'd at 7 A.M.	63·3 per ct.	57·3 per ct.	57·7 per ct.
“ “ “ “ 2 P.M.	69·5	55·6	56·3
“ “ “ “ 9 P.M.	47·3	44·5	43·0
“ “ “ for Autumn.	60·0	52·5	52.3
Rain—Amount, . .	13·111 in.	6·601 in.	10·705 in.
No. of days on which Rain fell,	34·	24·	27·7
Prevailing Winds—Times in 1000,	s.83°48′w·253	n75°50′w·201	n78°13′w·228

*Sky, one-third or less covered at the hours of observation.

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